AFCRL-67-0266
APRIL 1967
SPECIAL REPORTS, NO. 63

AIR FORCE CAMBRIDGE RESEARCH LABORATORIES
L. G. HANSCOM FIELD, BEDFORD, MASSACHUSETTS

# The Rayleigh Archives Dedication

J. N. HOWARD, Editor

OFFICE OF AEROSPACE RESEARCH
United States Air Force

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# Foreword

On 30 March 1966 a ceremony was held at the Research Library of the Air Force Cambridge Research Laboratories, Bedford, Massachusetts, to dedicate the Rayleigh Archives: a collection of the notebooks, manuscripts, working papers, and correspondence of the Lords Rayleigh (John William Strutt, the third Baron Rayleigh, and Robert John Strutt, the fourth Baron Rayleigh). The ceremony was attended by a distinguished group from both the military and the scientific community whose research interests relate to the two Rayleighs. The Strutt family was represented by the Honorable Charles R. Strutt (a son of the fourth Baron) and his wife. This report is a transcript of the discussions of "Rayleigh Day," with only a minimum of editing to correct minor errors that occurred in impromptu remarks. Appendices listing the attendees at the dedication ceremony and summarizing the contents of the Rayleigh Archives are included.

## PROGRAM

### DEDICATION OF THE RAYLEIGH ARCHIVES

Wednesday - 30 March 1956
Air Force Cambridge Research Laboratories - Research Library
Laurence G. Hanscom Field, Bedford, Massachusetts

Welcoming Remarks

Colonel Robert F. Long, Commander
Air Force Cambridge Research Laboratories

Brigadier General E. A. Pinson, Commander
Office of Aerospace Research

**DESCRIPTION OF THE MATERIAL PERTAINING TO THE THIRD BARON RAYLEIGH**

Dr. John N. Howard
Chief Scientist, AFCRL

**DISCUSSION OF THE SCIENTIFIC WORK OF THE THIRD BARON RAYLEIGH**

**DISCUSSION OF THE PROFESSIONAL ACTIVITIES OF THE THIRD BARON RAYLEIGH**

**LUNCH**
Officers' Club, L. G. Hanscom Field, Bedford, Mass.

**DESCRIPTION OF THE MATERIAL PERTAINING TO THE FOURTH BARON RAYLEIGH**

Dr. John N. Howard
Chief Scientist, AFCRL

**COMMENTS ON THE INFLUENCE OF THE FOURTH BARON RAYLEIGH ON AIR GLOW AND AURORAL RESEARCH**

Professor Sydney Chapman
University of Alaska

**DISCUSSION OF OTHER RESEARCH OF THE FOURTH BARON RAYLEIGH**

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# THE RAYLEIGH ARCHIVES DEDICATION

Frontispiece: The Royal Society Portrait of the Third Lord Rayleigh, Painted by Sir George Reid in 1903

# WELCOMING REMARKS AT RAYLEIGH DAY CEREMONY
# MARCH 30, 1966

## Dr. John N. Howard, Chief Scientist, AFCRL

When we first tried to decide what kind of ceremony we could have that would be appropriate for the dedication of this collection of archival material, it seemed to me that gathering together some Rayleigh people, scientists oriented in their research or interest to both Lords Rayleigh, would be a very appropriate way of doing this.

I have purposely tried to keep this entire meeting informal, although we still have to have introductions and stay on time and we have to stop for lunch on time. But if you look at the agenda in the program you were given, you will see that there is room for discussions, and several of you have said that you might be willing to discuss a certain aspect of Lord Rayleigh's work for a few minutes but, other than that, we will try to operate this meeting more or less as a town meeting or a Quaker meeting. If the spirit moves you to say something, we hope we have kept the number of people small enough so that this is practical.

Arranging this sort of meeting was a little tricky as there are a lot of people nearby who would like to participate, 1200 of them being right here at Air Force Cambridge Research Laboratories. Because we did not want this discussion group to be more than 50 participants, I have had to be rather unkind to our own people. I promised them that later we would have a separate internal ceremony without the invited guests. But to begin our ceremony, I would like to introduce Colonel Long, the Commander of AFCRL, to give some welcoming remarks.

## Colonel Robert F. Long, Commander, AFCRL

To continue Dr. Howard's stated air of informality, I will take only a few moments officially to welcome you to L. G. Hanscom Field and to the Air Force Cambridge Research Laboratories. I think that our good fortune in having acquired the Rayleigh papers is exceeded only by our good fortune in having this illustrious audience here today to participate in the dedication. We are extremely indebted to the Strutt family. We are also indebted to our friends in the Boston area here, both industrial and academic, for helping us put together our program and for making a facsimile of some of the papers for us. We are glad to have you with us. I will now turn the meeting over to General Pinson, the Commander of the Office of Aerospace Research.

## Brigadier General Ernest A. Pinson, Commander, Office of Aerospace Research

Mr. Strutt, Dr. Howard, Distinguished Guests.

The Air Force is truly honored that you could be present here today to participate in the dedication of the Rayleigh Archives at the Air Force Cambridge Research Laboratories.

The establishment of these historical archives is an event almost without precedent in a military research laboratory – and in a special sense marks the coming of age of the management of science by the Air Force. The deep pride that we take in the establishment of these archives at the Air Force Cambridge Research Laboratories is a measure of the homage we pay to science in its own right – to its premises, and to its heritage.

The scientific administrator who does not honor the distinct set of values of the community of science, who looks upon science only as a rich resource to be mined for technological need, will most assuredly defeat the very goals of his administration.

We of the Air Force fully realize our responsibilities in the establishment of these historical archives. We do not take these responsibilities lightly. An archival collection is a trust. It is indeed a trust which is undertaken for all mankind. As you examine the documents in this archival collection today, you will, in a very real sense, examine points in history where man's progress toward greater enlightenment are marked.

Dr. Howard will later comment on the background of this collection, on his progress, as curator, in cataloging the materials, and on his efforts to make the contents available to a broad public. As a speaker on behalf of the Optical Society of America, he has during the past year made some 50 presentations at university seminars and to various professional societies on the Lords Rayleigh and on this

archival collection. He has made arrangements for their publication, has explored possibilities for making the documents available to Ph. D. candidates as thesis background material, and has reproduced facsimilies of the original material which he has sent to the archives of the American Institute of Physics and the Library of the Imperial College, London.

During the course of the day, you will have the opportunity to tour our library here and you will be, I am sure, as so many other visitors to our library have been, greatly impressed with the scope of our collection and with many of the historical documents. We are particularly proud of three of our serial collections. You will see the complete set of Philosophical Transactions of the Royal Society of London dating back to 1665, a complete set of the Histoire of the Paris Academy dating back to 1699, and a complete set of the Commentarii of the Russian Academy of Science dating back to 1726.

Our collection of geophysics literature is one of the most extensive of any technical library in the world. Lastly, in your tour of the library, you will surely be surprised at the large number of foreign periodicals available to scientists not only at AFCRL, but to other researchers in the Boston scientific community. The periodicals received from behind the iron curtain—including Red China—may be of special interest. But current literature, no matter how extensive, does not make a fine library and can never impart the aura of scholarship upon which scientists place such high value.

The Air Force Cambridge Research Laboratories, which last year celebrated its 20th Anniversary, has in this brief history made many contributions to the advancement of science.

Each year, AFCRL adds scores of small accruals which, in the aggregate of similar contributions by researchers the world over, provide the foundation—and indeed make inevitable—the heralded break-throughs.

A brief history of 20 years, and a record of many contributions to science, however, can hardly qualify these Laboratories for the Mantle of "institution." In the tacit and guarded way in which we so elevate our organizations, I am not so bold as to assume that AFCRL possesses the essential credentials. Our ambitions are far more modest—but I think we have obtained them in large part. The Air Force, through the existence of its fine Laboratories here at Bedford, Massachusetts, is no longer an intruder into the community of science, but has gained acceptance as a fully participating and contributing member.

Through this fine historical collection, which we will dedicate here today, there is the token of a covenant which we share with the scientific community—of a subscription to mutual values.

Thank you.

# DESCRIPTION OF THE MATERIAL PERTAINING TO THE THIRD BARON RAYLEIGH

DR. HOWARD: Thank you very much General Pinson. I am afraid in his discussion of the collections in the library, General Pinson scooped some of what I was planning to say, but that will simply shorten my material.

What I plan to do next is the third formal part of the agenda: I want to give a short description of the material we have here. Yesterday we put a lot of manuscript material out on display, and we tried to put up also some pictures and letters. Some of these are souvenirs from the Rayleigh family that I have borrowed for this occasion, and I might mention that Mr. Strutt has also brought some souvenirs with him which we can show off today.

Here are pictures of Rayleigh's medals: first his Nobel Prize, Lord Rayleigh 1904. This medal I am not so familiar with, although I read of it in the life of Rayleigh; it is the Barnard medal, "For meritorious service to science," awarded by Columbia College in 1895 to Lord Rayleigh for his discovery of argon. It is a bigger piece of gold, but certainly not quite as well known as the Nobel Prize. Here is another very impressive medal—the Order of Merit of Edward the VII. This was an order in .tuted by Edward the VII, and the first 10 or 11 designees were awarded this medal during the celebration of his coronation. I once looked up the origin of this award in the Illustrated London News and saw a group picture of 10 or 11 recipients. I am told that Florence Nightingale was in the first set; the only other scientists beside Lord Rayleigh in the first one were the astronomer, William Huggins, and Lord Kelvin. I think Rudyard Kipling was also in the first set. This

Order is an extremely exclusive club—limited to 25 or so people. Winston Churchill barely made it because, first of all, it is not supposed to be awarded to political people. He made it for his writings. I think in the case of Churchill the other problem was that there was no vacancy until shortly before his death. These are certainly remarkable souvenirs of a great man.

Mr. Strutt also brought some nice aerial views of Terling. There is a picture on the wall of Terling, the country estate in Essex of Lord Rayleigh, where the original stables had been converted into his private laboratories. If you look closely off to the left in that picture, you can see a little observatory dome that was not added until 1930. They had had a little fire in the attic, and in repairing the roof they took the opportunity to add an observatory dome so that Robert Strutt, the fourth Lord Rayleigh, could make his night sky and auroral measurements a little more conveniently from inside. Terling is a rather impressive place.

Well, first I should give some accounting. Most of you are aware of how this particular collection was acquired. The reason you are here is because you have made yourself known to me as people interested in Rayleigh. I sent you reprints of a special issue of Applied Optics in October of 1964 that was devoted to Lord Rayleigh. There was one paper in that issue in which I made an effort to describe the circumstances by which we acquired this collection. But to repeat it very briefly and to correct a few points, it was our interest in the fourth Lord Rayleigh, Robert Strutt, the airglow Rayleigh, that really prompted this acquisition.

That was because here at Cambridge we have a very active group studying the upper atmosphere, the Upper Atmosphere Physics Laboratory, and in that group is a branch called Airglow and Auroral Physics. This group was endeavoring to analyze, digest, and publish the results of a year and a half of data that had been taken on the earth's aurora and airglow during the IGY, the International Geophysical Year. Specifically, Dr. Silverman and Dr. Hernandez were digesting this material and trying to prepare an article on it for the Journal of Geophysical Research.

They had data for wide variations of latitude, longitude, time of year, places from the North Pole to the South Pole, and so on, but the one parameter they did not have was a proper time variation. No data existed for going systematically back earlier than 1950, say, which is roughly when we at AFCRL started to take such data. Other groups at Saskatchewan and Alaska became consistently active in taking airglow data around 1950. Almost the only earlier data in the literature were the occasional data that Robert Strutt, the fourth Lord Rayleigh, had observed, mostly from Terling, starting around 1920, intensively for a while, and then sporadically until his death in 1947. Considering this, Dr. Hernandez mused: wouldn't it be nice if we could examine Rayleigh's original data and perhaps correlate this more closely with our own, because (as I understand it) Lord Rayleigh had not really published all of his data.

He had published "monthly mean averages," but the averages had erased what he thought were minor variations; we would like to include these minor variations in making a proper correlation. In fact, Dr. Hernandez and Dr. Silverman felt that if they could have more extensive data, they could actually make a correlation with an entire sunspot cycle, a 22-year cycle. While Dr. Hernandez was musing about this, he picked up the telephone and called the Boston British Consulate and asked: "How do you suppose we would go about finding original material or notebooks of the fourth Lord Rayleigh?"

The Consulate referred the entire matter to the British Information Agency in New York City, which wrote back that this particular Lord Rayleigh had been a Professor at Imperial College, had been on the governing board of the National Physical Laboratory, and had been a long-time Fellow of the Royal Society. They suggested we ask the libraries of those three sources for his scientific material, and they suggested also that since we are an Air Force organization and there is an Air Attache in London, why not let him do the searching for us.

Dr. Silverman wrote a letter to the Air Attache in London, who was a Brigadier General and not particularly oriented towards airglow research. This request overwhelmed him a little bit, I think, and he immediately forwarded the request to the Office of Naval Research which maintains a technical liaison office in London, consisting of a dozen people or so, who go to technical meetings, write up little resumes of what is going on in Europe, technologically speaking, and so on. ONR in turn gave the request to their librarian, a Mrs. Griswold, now remarried and now Mrs. Hewitson, and asked her to fill this request from the Air Force Cambridge Research Laboratories.

She started out by writing a letter to the Royal Society: "Do you have any material of Robert Strutt, the fourth Lord Rayleigh?" They replied, "No, nothing in our archives—a few referee reports, but nothing substantial." So then she wrote the National Physical Laboratory and their librarian wrote back that they also really had nothing. She then wrote the Lyon Playfair Library at Imperial College, which is the library for the history of science, and they also said they had nothing. However, this exchange of letters intrigued them a little bit because each of these organizations had assumed that one of the other places had such material.

One of the younger librarians, Geoffrey Paterson at the Lyon Playfair Library, was curious enough to write a letter to one of the sons of the fourth Baron. He wrote to Mr. Guy Strutt, who responded that he and his brothers were a little unhappy to admit that none of them were scientists or scientifically inclined, and when their father had died, they had closed off the laboratory, turned off the heat and electricity, and they didn't really go there very often. But in looking in a few years later, they had noticed that in the bookroom, for example, where most of the manuscript material was, including complete sets of such journals as Phil. Mag., Proceedings of the Royal

Society, and so on, things were beginning to mildew. They were unhappy about this as they felt there might be some value in collections of this sort.

They had an appraiser come out from London who looked at the library and said there were a lot of people who would be interested in this. So they had an auction and disposed of the material and, more or less for good measure, I suppose, threw in a small trunk containing 33 notebooks, handwritten experimental notebooks.

At the auction, Dawson, a book dealer in London, had acquired these and had put them in his basement. I am not sure whether he had ever really listed these notebooks in his catalogues, as our cataloguers regularly examine such lists and we hadn't noticed this item. This information about the notebooks was reported back to us, and somebody went over and inspected Dawson's basement. Sure enough, there was this trunk of notebooks which Dawson was unwilling to break up, but which he was willing to sell as one item. So we acquired this, hoping we would find enough original material to make a correlation of airglow data.

The notebooks arrived here at AFCRL in the fall of 1963. As we unpacked it we found, sure enough, we had 22 notebooks, practically the entire experimental notes of Robert Strutt, the fourth Lord Rayleigh, starting from roughly 1904 or 1905 and continuing until almost 1947 when he died. (We later acquired from the family one additional notebook, which was subsequent to the others.) Here were his complete data on all of his experiments—airglow, active nitrogen, all of his experimental activities, and I must immediately say that we have already been able to make some use of this: Dr. Silverman has published an article in the Journal of Geophysical Research making a preliminary comparison of Robert's data with others. We certainly hope to do more of this.

As we unpacked the notebooks, we were astounded to find underneath these 22 notebooks of Robert Strutt, 12 additional notebooks—11 of which were in the handwriting of John William Strutt, the third Lord Rayleigh, the father of the fourth Rayleigh.

It is J.W. Strutt that most of us in studying physics think of as Lord Rayleigh because he was the very general physicist who excelled in all areas, all disciplines of physics. Here were his notebooks from his first lecture notes at Cambridge, his optics lectures under Stokes, and his experimental notebooks. There was no theoretical material because, as we will see when we examine his papers, when he felt theoretically inclined, he sat down and wrote a paper and then sent it off to a journal—he didn't do it first in draft form—so only the experimental data got put in a notebook, which he needed to save until he was ready to incorporate the data in a paper. In addition to the 11 notebooks of John William Strutt there was a notebook in the handwriting of his sister-in-law, Eleanor Sidgwick (who had collaborated with him on his capillarity studies and on his electrical studies when he was Cavendish professor in the period from 1880 to 1885).

That is how we acquired the Rayleigh notebooks. And now I must immediately say that it was the curiosity, you might say, of Drs. Hernandez and Silverman that started the ball rolling. It is certainly equally true that we have not publicized the fact that it was our library staff, particularly John Armstrong in acquisitions, who recognized that this manuscript material would be a juicy plum to acquire; and it was our librarian, Ole Groos, who is really the man responsible for us having these splendid collections that General Pinson enumerated: Phil Trans. Roy Soc right back to its first year, and similar collections of serials. In fact, you can name any learned society in physics, mathematics, astronomy, or geophysics and we have managed to acquire their journals back to the beginning. By horse trading, by every honest means we could, perhaps even right to the edge of honesty, I think. This is remarkable when one considers that we only started in 1948.

It was in the fall of 1946 that the Cambridge Field Station was set up, but the library didn't start until 1948. I could name several other libraries of big defense organizations that also started in 1947 or 1948 and have library budgets as big as or bigger than ours, and have buildings even more handsome. I don't want to name them, but if you go into their stacks and look at their serials, they have almost nothing that goes back earlier than 1945. They just subscribed immediately to current periodicals and they have no roots; they have nothing that goes underground. Of course, they can come here to our library if they have to go further back, I suppose. But we are very proud of the fact that an extremely alert team of librarians determined to assemble a good overall collection, and a man clever enough to recognize a good acquisition, have been able to build the AFCRL Library. The stacks, by the way, are right back through that door: physics is way in the back of this floor, mathematics just ahead of it, and then upstairs, learned societies are in the front, Comp. Rend., Phil. Trans. Roy Soc, and so on in the front, and then astronomy and geophysics further back.

We have also other remarkable collections: the accounts and papers of many famous explorers, such as Humboldt or Sir Hubert Wilkins, who as I recall died here in Natick, Massachusetts, not very long ago. When Chalres Brooks died, who had been the long-time director of the Blue Hill Observatory of Harvard, we acquired a large segment of his library. Our interests are very much oriented towards physics and geophysics and selected electronics.

Although we have only 250,000 books, these are in three very narrow specialties. We have also another 150,000 government and contractor reports also in these same specialized areas, so altogether I think we have an extremely high quality collection. I have looked at department libraries of every big university in this country and we can beat them all: true, we can't match the Widener Library at Harvard, but we can beat their Physics Library, for example. But well, enough of that. That is how we first acquired the collection, and I was not involved in any of this until that point.

At that time I was editor of a journal for the Optical Society, and when I heard via one of our little internal AFCRL weekly bulletins that we had acquired some material of Lord Rayleigh, this to me rang the bell of optics. Now to many of you it rings the bell of acoustics, or any other field that you happen to be working in, but we have a good claim in optics. If you look at Rayleigh's complete works, 446 papers, the biggest single number of kinds of papers were classified by Rayleigh himself as optics, something like 150 of them, while only 140 were acoustics. Fortunately, he didn't count the "Theory of Sound," however, which would throw the scales the other way. But nevertheless, I went over to the library and inspected that material and thought we have got to do something about this: this isn't something to put in the stacks and forget about. I immediately started in the mill an effort to schedule an issue of Applied Optics devoted to both the third and fourth Lords Rayleigh, and I wrote around to some people working in atmospheric optics, in airglow, and auroral physics. We soon had what looked like a very nice issue shaping up.

Then I made a visit to Terling to show the Strutt family what my plans were because I didn't want to do anything that might embarrass the family; for example, they might not particularly want the family estate to become a tourists' attraction. I first wanted to see that they had no objection to the Applied Optics feature, and I found they were really very pleased; particularly pleased that AFCRL had an interest in their father as well as in their grandfather, as they very understandably felt that when there is such a father and son combination and the father was a mammoth, you might say, that he rather overshadowed the son. The fourth Rayleigh, if he were standing alone with his 300 publications and his contributions to airglow and gaseous discharge theory, would be considered a more significant figure if he were not standing in the shadow of his famous father. So they not only cooperated, but they astounded my by mentioning that, by the way, they had in the attic some other material of grandfather that might interest me. And they proceeded then to bring down from upstairs those three metal boxes that you see over there, stuffed full of wrapped up materials and manuscripts.

I spent an hour taking a hasty look at some of these, trying to see what they were, roughly jotting down titles of a few of them. Then I hurried back, all out of breath, on the next plane and told Ole Groos, our librarian, and John Armstrong, in charge of acquisition, about this new material, and again they started the very delicate sort of process of seeing if we couldn't also acquire this. Delicate in the sense that we are a military organization and we have a little bit of trouble persuading people—they all don't have Ph. D. 's like General Pinson—persuading them that science requires roots as well as what shows above ground and that it is proper to have some historical perspective and this was proper material for us to acquire. But this very delicately went through the procurement mill, and in the fall of 1964

or early 1965 these boxes arrived here and I started to analyze the contents in a more systematic way than I had been able to do before. This constitutes the bulk of the material that we now have. Actually there is much more material here than in the notebooks.

I would now like to describe the material of John William Strutt, the third Lord Rayleigh, and defer the discussion of the other material until, say, after lunch, when we can turn to the fourth Lord. But first of all I might say that most of this had been folded up, rather tightly folded up, for from 50 to close to 100 years and resisted being unfolded, and I was determined to microfilm and copy it.

When General Pinson said we are almost unique in having a collection of this sort, the one exception that comes to my mind is the Michelson collection. Michelson was a Navy man: he did some of his first work on the velocity of light at Annapolis and then he was at the Naval Observatory before going to Case. When he died, he left his collection, or a large part of it anyway, to the Navy and they put it at NOTS, the Naval Ordnance Test Station at China Lake, California, which is not far from Bakersfield, 90 miles out in the country from Los Angeles. It is a beautiful collection. I have been out there a couple of times and seen it. It contains his Fourier generator, his ruling engine, diffraction gratings, his interferometer, plus his Nobel prize in a glass case, and things of that sort. But not long ago, another old Navy man, Frederick Seitz, who is president of the National Academy of Sciences, began thinking that it is not right to put a nice collection of material of that sort 90 miles out in the country where nobody has easy access to it, even though the Navy had a claim on it. I understand that the collection was packed and brought from China Lake to the Naval Observatory in Washington where it can be displayed in a more accessible place. One can appreciate this philosophy. But we feel that Boston is equally accessible: we are not 90 miles out in the wilderness; we are only 15 miles out in the wilderness.

Just to make doubly sure that no one accused us of taking a treasure and burying it, my first desire was to get this material copied and microfilmed as quickly as possible. As rapidly as it has been practicable to do so, I have deposited a complete set of microfilms at the Lyon Playfair Library of Imperial College, which, after all, was the library that helped us to acquire this. Robert Strutt, the fourth Lord, was a professor there a large part of his life; and, as a matter of fact, he was Chairman of the Board of Governors at the time of his death. So he had very deep involvement with that college. We have also put some of his airglow material at Belfast, Northern Ireland, where David Bates is the airglow man, and we have tried also to put a complete set at the Neils Bohr Library for the History of Physics in New York City. We are also depositing the airglow papers of Robert Strutt in the library of the National Bureau of Standards at Boulder, Colorado, to serve researchers in the NBS, ESSA, the National Center for Atmospheric Research, and the High

Altitude Observatory of the University of Colorado. So I hope we can soften the complaint that the U. S. Air Force should not have this material, that it belongs in the British Museum.

But we acquired it; we feel a great affinity to the airglow data and research. We feel also that John William, the third Rayleigh, had much interest in the atmosphere: his first famous paper was an explanation of the mechanism of the blue of the sky (which we now call Rayleigh scattering). We have a very active Rayleigh scattering group right here at AFCRL. So we feel an affinity to both Rayleighs, and we don't feel that we are hiding the material. There is no light under a bushel here; we are trying very definitely to exploit this material in a proper way.

The first practical project I had to do was to iron these manuscripts. They would not stay flat even to photograph them. I took a briefcase full of them home at a time and ironed them with my wife's steam iron, at the low heat for synthetic rayons. I experimented on something I thought was worthless scrap, or almost worthless scrap, and found that ironing didn't hurt the ink or the paper.

Rayleigh used pen and ink until about 1890. Then he must have got tired of dipping that pen and changed to an indelible pencil, a pale blue indigo pencil, which is very hard to read, especially when the paper gets aged and starts to turn brown. But in a way photographing it helped because it is more legible in the photographic reproduction than it is in the original. Fortunately, on the manuscript of his published papers he used a reasonably high-grade paper; it looks like a rice paper of some sort. For his rough notes, on the other hand, he used anything available.

Having ironed the papers, I then started to sort them. Fortunately, just about that same time Dover reprinted the complete scientific papers. Rayleigh himself had supervised the printing of the first four volumes of his "Scientific Papers" between 1899 until 1913 or so, and Volume 5 had appeared just before he died. Robert, his son, brought out Volume 6 in 1920 at the University of Cambridge Press but the entire set has been out of print now for some years, but you can still occasionally get Volume 5 or 6.

Several of you here were instrumental in persuading Dover to bring the "Scientific Papers" back into print. Dr. Victor Twersky was probably the most active; he bombarded Mr. Cirker at Dover with arguments that a collection of Rayleigh's papers is too valuable to keep in a rare bookroom: it belongs on everybody's desk. Bruce Lindsay backed him up, and I am told that Professor Donald Menzel also wrote concerning Rayleigh. All of this combined to make Dover decide to gamble on this. And it is a gamble in a sense that Mr. Cirker of Dover is an alert businessman and does a nice series of scientific books, but he has stuck completely to text books and reference material.

He once told me that the one time he wandered into the history of science he picked what he thought would be the most popular Newton biography for the occasion of Newton's tercentenary and it just didn't sell at all. It scared him a little bit,

but he felt that he would make a try on one of these sets of complete papers. He felt if anyone would sell, Rayleigh possible would, because as you know Dover brought out in 1942 the American version of the "Theory of Sound" and that has since stayed continuously in print. In fact, now it is a paperback on technical book shelves and it is still one of Dover's best items. It made Mr. Cirker feel that Rayleigh wouldn't really be a bad gamble. I think he printed for the first printing something like 1500 sets; his break even point was something like 1000. The last I heard, some time ago, was that he had sold 700 sets so far. So I urge you to get all your friends to buy a few sets. If Dover finds that publishing Rayleigh's scientific papers was not a bad investment, perhaps they would publish the complete works of other worthwhile scientists, such as Stokes.

Well, this Dover set came out just about that time and, in fact, I was able—by adding my voice to Twersky and Lindsay—to help persuade Mr. Cirker that he should add a little supplement of some of the Rayleigh material that we acquired, and also a short Rayleigh bibliography which we contributed. Just think, not only are the complete scientific papers of Lord Rayleigh again easily available, but they are available for only $30.00 for 3700 pages—that is considerably less than a penny a page. As publishing costs go these days, for a hard-bound book that is one of the biggest bargains I can think of.

Well, this set had just come out and that made my work much easier because I was then able to take that box of "published manuscripts" and correlate them with the published papers.

These "published manuscripts" consisted largely of the drafts of Rayleigh's theoretical papers. After working out derivations on scratch paper, Rayleigh would sit down and write the manuscript at one sitting. He would then send it to the Phil. Mag., which was his favorite journal. Phil. Mag. then returned this manuscript with the galley. Most of us writing a paper nowadays would throw the original draft away after we received the galley, but Rayleigh put many of them aside and saved them.

All one has to do to identify these drafts is to look at the title or go through the subject index of the "Scientific Papers." I identified these drafts by the number of the published papers. One of the inserts in your little folder is a listing of the ones that I have identified so far—187 of them.

Rayleigh didn't save very many of his early manuscripts: only 6 of the first 200. But one of these was paper Number 8, his first famous paper: on the blue of the sky. He knew even when he wrote that in 1870 that it was one he was going to be proud of; I found the manuscript of it, rolled up in a tight little bundle, tucked down in the bottom of this box. We managed to get it to lie flat with a steam iron, and it is now in beautiful shape.

The nice thing about having his original drafts is that you can see where he decided to change a word, change the order of a page, or scratch our a paragraph. These are not really important changes because you will find he didn't make very many changes like that, but paper Number 8 is a very good illustration of the early Rayleigh.

Maybe we should stop here and mention the extremely generous contribution to our ceremony of Dow Smith of ITEK, our neighbor here in Lexington. When he heard that we were organizing this ceremony, he said ITEK should do something as a contribution to the cause. Dr. Smith had ITEK reproduce paper Number 8 for use as a party favor a hand-out you might say, for this occasion. The only short cut they took was to print on both sides of the page, whereas the original was written only on one side. Everything else is authentic: the page size is identical, and the water-mark and even the blemishes were copied. If you look at it under a glass you will see it is a screened reproduction, but you can't tell that if you just casually examine it.

Perhaps what gave Dr. Smith the inspiration, the idea to do this, was the splendid reproduction that Honeywell had done of Michelson's 1879 notebook on the velocity of light. They did it because their Vice President for Research, Dr. Finn Larsen (now again in the Pentagon), was very interested in the history of science. When Dr. Larsen saw the Michelson notebook on display at the Naval Observatory—a beautiful set of 100 determinations of the velocity of light done by Michelson in longhand, complete with his calibrations and his corrections—he arranged for Honeywell to reproduce it through the Lund Press in Minneapolis. It looks so authentic that when you look at it you think you have the real thing. Well, this gave Dow Smith the idea that ITEK ought to be able to do an equally nice job on one of Rayleigh's papers. Paper Number 8, which you have there, I think is done with the same care and the same success as the reproduction of Michelson's notebook.

Remember that the Michelson notebook was in very fair shape, without any corrections or changes—he had recopied it once I believe. The Rayleigh paper was the first-time original manuscript, just as he sent it off. He didn't know that anybody, nearly 100 years later, was going to reprint it. So the little minor mistakes in it are a good indication that he did pretty well the first time.

There is one amusing thing on the title page in longhand. It says "On the light from the sky—its' polarization and colour." Notice he put an apostrophe on "its," but that is simply because he was not an English major: he was a physics major. When Phil. Mag. printed it in the February issue of 1871, they took the apostrophe off. The title cover on your booklet is adapted from the printed 1871 Phil. Mag. ITEK used the same print font of that journal for the cover of the booklet.

I was worried when I saw this title page: we had spelled polarisation with a z when an Englishman would spell it with an s. If you look at his handwriting you

can't quite tell whether it is an s or a z. But I looked at the 1871 Phil. Mag., and it is spelled with a z there; maybe it is only nationalisation that they spell with an s. But anyway, ITEK is accurate and I believe that this is a very fine favor and we are deeply indebted to ITEK for having done this. I hope you all treat it with proper respect and remember that it is a scarce item. If you don't want it, well, we know several other people who do, so don't throw it away.

Now, I might say, looking at the complete papers, that I have ironed them out and sorted them into this file cabinet here. I would like you to take a look at them. But if you take anything out, please put it back where you found it because these are not all microfilmed yet. They are in folders identified according to the paper number. In this cabinet there are 187 identified papers plus 5 others; 3 of which were found by Robert after his father had died. Robert had them printed, and they are now included in the complete works. The other two Rayleigh had published in journals, but decided not to include them in his bound scientific works; therefore, it is not possible to identify them with one of these numbers.

We also found his earliest work, which is on display in the little glass case by the library entrance, dated 1865. It is 4 years earlier than anything he published in Phil. Mag. This was a privately printed lecture on photography that he gave in Essex at some local event. It is interesting because, you must remember, photography—wet plate photography—had only been invented in 1851, so in 1865 that art was only 14 years old.

Photography was one of Rayleigh's very first hobbies, and some of his early papers reflect his efforts to try to reproduce diffraction gratings, to make them finer by reducing the size photographically, and also to make replication of gratings by photographic contact. My own feeling as to why there are so few of the early manuscripts (Why are there only 6 drafts of the first 200 papers and over 75 percent of the drafts of his remaining 246 papers?) is that until he wrote his paper on argon, paper Number 214, Rayleigh, being a well-to-do man, had no real interest in priority or in patents and things like that. I don't think he ever took out a patent in his life. He was the opposite of Kelvin. I think Kelvin ran to the patent office first and then to the journal. But Rayleigh didn't really realize some of the furor and uproar you can get into over priority until the argon paper was being written. Then a lot of people felt that Ramsay did most of the work, some credited Rayleigh, and Dewar made unhappy comments. From that time on, Rayleigh kept everything—he never threw a thing out from about paper 200 on. But by that time, the earlier ones had probably already been tossed.

I still expect to rescue another half dozen or so of his drafts of early published papers. He wrote on only one side of a page, and he used the empty side of other paper for his rough calculations. I have found on the back of quite a few of his later papers incidental pages from earlier papers. After I get them all microfilmed, I

think I can collate them and assemble three or four more early papers. Several of the pages I recognized as being from his 1885 "Encyclopaedia Britannica" article on light, for example, which would be interesting to reassemble. Well, so much for the discussion on the published papers.

I would like to give a quick account of the papers that are called mathematical manuscripts. These turned out to be his rough calculations that went into the published papers. Now Rayleigh did not have an IBM computer nearby and he did not even use a slide rule. He leaned completely on tables of logarithms. Anytime he had to do a calculation, he worked it out in logarithms. If it was a messy function, he would expand it somehow and discard terms higher than he needed. If it was something he could do with Bessel functions or Legendre polynomials or any such functions, he would generate the term he needed. These were not tabulated either, in those days, except for the very lowest orders, but he would take a recursion formula and sit down and generate this function.

If he wanted the 20th order Legendre polynomials (and they were only tabulated to about 8 at that time), he would sit there and patiently generate each successive term, sometimes using 30 or 40 pages to get the term he needed to make one little point in a table. He saved all these calculations, which I'm trying to sort. Its a rather hard job for me to sort, because sometimes they are not dated. So far I have identified roughly 100 of them according to the paper that they relate to. I have identified another 35 or so by the year that they relate to, but I am not quite sure which paper they relate to. I know they have to be close to that year, because you can tell this by the material from the other side of the page.

As I said, Rayleigh was a thrifty man, perhaps stingy is a more accurate word. I don't think he ever spent a penny for a peice of paper, except when he was writing a manuscript to send off to a journal. He must have bought that paper and wrote on one side, and he bought it in quarto and sliced it with a knife because I found several of them that matched together along the slice. For journal manuscripts, he wrote on only one side, but all of the other calculations are on saved paper.

You can date many things from his habit of using the back side of used paper. His brother-in-law, Francis Maitland Balfour, was a professor of plant morphology or animal morphology at Cambridge, and every time that Rayleigh went there he grabbed a handful of students' papers. Since students were allowed to write on only one side, Rayleigh used the other side. While he was Cavendish professor, he wrote on the back of student assignments in physics. Later, when he was on many committees, he used the back of committee reports. I was able to identify the years on some of these.

The rough calculations that I have identified vary from just a few pages to as long as 222 pages of calculations of functions. Here is one calculation on the back of a report from the Advisory Committee for Aeronautics: "I enclose for your

information a copy of a letter received from the War Office dated 24 July . here is a National Physical Lab report; here is one that says "strictly confidenti (but let me immediately tell you that that kind of confidential meant that they were discussing somebody's salary or something of that sort).

I even found that one could identify some of the people who had written on the other side. Glazebrook, for example, was doing assignments for a while, and some of Rayleigh's rough notes are on the other side of these. William Napier Shaw was one of his assistants at Cavendish, and there are many of Napier Shaw's assignment papers. (Napier Shaw was later the first director of the Meteorological Office and became Sir Napier Shaw.)

There is a lot of what we today would call junk mail. Since he was a Lord, a member of the House of Lords, he got all kinds of implorings to vote this way or that way on a certain bill for Russian war relief or something like that. Some of this is very interesting. Here is a letter from Baden-Powell: "Would Rayleigh support the boy scouts?" If a letter had a blank side, Rayleigh saved it.

I have listed for you on another hand-out the numbers of the 100 sets of calculations that I have so far been able to identify. Now, here is another drawer full that I have not yet been able to figure out; I can't even figure what year it is. I can tell if it is early because it is in ink or if it is late because it is in pencil, and one can get some idea of the subject. But so far, I have not been able to relate these to anything in the published works. Remember, this is supposed to be a hobby with me; it only takes 130 percent of my time, I think, but nevertheless, there is still a lot yet to do.

Now, let me mention some of the other material that is not on this list, just to give you a feeling of what we have. Here is a very interesting paper, one of his first studies—the subject that started him on looking at the blue of the sky—on human color vision. He was examining all of his relatives sitting out in the sunlight in front of Terling, using a little Maxwell color wheel, and he described a little summary of this work in paper Number 7. Here, in the rough notes, are the pieces of colored paper that he cut his disks out of. If we wanted to repeat those experiments today, well at least the same authentic colors are there. He just called them red, orange, yellow, and blue, but this is an interesting souvenir.

Now, I might say also that he first fastened his pages together with pins; then, about paper Number 100 or 150, he used these paper fasteners that you bend over. Paper clips appeared later, nasty old rusty things, that I had to get off, and finally about paper 350 one finds staples. That must have been about when they were invented. (The staples were added by Phil. Mag. when they received the manuscript; I am sure Rayleigh didn't have a stapler.)

Once we got started on this Rayleigh collection, John Armstrong really went after the other material that had been auctioned off at the same time and was still

available. Several of those volumes are on display at the end of this table. For example, there is the complete set of the works of Stokes that Stokes presented to Rayleigh with his compliments. Rayleigh has made marginal notations here and there where he disagreed with something. Several volumes that had been owned by Rayleigh (and also by the fourth Rayleigh) are on the end of the table. Well, we have also the German edition of the"Theory of Sound," which I had never seen before, dated 1895.

In this last file cabinet, I have sorted out a few other oddments of things. Here are his Cambridge lecture notes when he was Cavendish professor: lecture notes on color vision, scattering, sound, electricity, magnetization, density of gases, elementary electricity, and the BA experiments on electricity. Each lecture is in a little folder. Each set is just a little envelope of pieces of paper with key words to remind him of what he was going to say next; it is a little bit hard to reconstruct what he said. Here is also an assortment of his Royal Institution lecture notes. These were a little more formal because you were supposed to give these Friday evening lectures during a series of weeks near Easter for the members of the Royal Institution. Here are notes for 1889 on light; 1890 on electricity; 1891 on forces of cohesion; 1892 on matter at rest and in motion; 1893 on sound; and in later years lectures on light, fluids, heat, capillarity, polarized light, sound, shadows, light and heat, and one on astronomy and one on flight in 1900.

In 1899 he gave one additional lecture on the occasion of the 100th anniversary of the Royal Institution, the centenary lecture on Young; this is rather interesting. It is completely written out because the Prince of Wales, later Edward the VIIth, was to be in the chair and Rayleigh evidently decided to check his time so he wrote the lecture out. The others are just rough notes. In fact, it starts out: "Ladies and Gentlemen." In looking it over he wrote in: "Your Royal Highness, Ladies and Gentlemen." "Your Royal Highness" was an afterthought; you can see he wrote it in the margin.

These lecture notes are of some interest, and I have been sending copies of them to the Librarian at the Royal Institution because all they had were short write-ups that appeared in Nature, I believe, and in the Proceedings of the Royal Institution, little one-page summaries of the lectures. But here we have the complete set of lecture notes.

Last summer I had remarkably good fortune. We had a last minute involvement with the Great Society, in that we were asked if we could hire certain unemployed college students under the Youth Opportunity Program, and I had working, a room away from me, a very bright young girl that they had sorting through some dead files just to keep her occupied. I found she didn't seem to have enough to do and I said: "Why don't you try typing up some of this material that I have?" I found she could read Rayleigh's handwriting better than anyone I have met. Perhaps it

resembled her father's writing, but she could read it and she was a good t. So I managed to get her to transcribe three or four of the notebooks and all of Royal Institution lecture notes and quite a bit of other material: all done surreptitiously because other people thought she was cleaning dead files. If that Great Society program is still continuing next summer, I'll try again.

Rayleigh had a very nice library, and whenever he read a book, if he disagreed with or wanted to check a derivation, he would do this on a slip of paper that he usually left in the book. His son went through a lot of these books and removed the notes and put them in envelopes. So we have notes on: Maxwell's "Heat," including quite a few recommended corrections; the "Theory of Sound," including corrections and things Rayleigh would do differently; Boussinesq's "Application of Potential"; Thomson and Tait; Gray and Matthew's "Bessel Functions"; Rayleigh's own "Scientific Papers"; Goddington's "Optics"; Lamb's "Hydrodynamics"; Shaw's "Forecasting of Weather"; and Bassett's "Hydrodynamics." All of them are here in folders.

Sir William Abney examined Rayleigh's eyes, and his chart of Rayleigh's color vision is over there. Notes for a lecture at Felstead school on the advantages of education; comments on Stokes' Wilde lecture on optics; comments on Callendar's "Theory of Radiation."

Among the unpublished notes there are several derivations that he started and then gave up on. For example, he tried to solve the theory of the collapse of cylindrical shells by using spherical harmonics. If one were doing an elasticity study on a cylinder, one might logically use cylindrical harmonics, and Rayleigh has published such derivations. But he just got curious: could it be any easier with spherical harmonics? And for 20 or 30 pages he got into some involved mathematics and finally wrote a little note that this is obviously leading nowhere, which was the end of that paper. There are two or three attempts of this sort: another one ends, "this won't do."

There are many papers for the period during which he was a more senior, older advisor to the Government: for example, he was made Chief Scientific Advisor to Trinity House which somewhat relates to our Coast Guard. They maintain the fog horns and the lighthouses around the coast. And since Rayleigh was a theory-of-sound man, he did quite a bit of designing of fog horns and also some designing for lighthouses. Here are some of his blueprints for "Lord Rayleigh's design of a fog signal" for one of the lighthouses. In the folder of this Trinity House material I even found a set of photographs that Oliver Lodge took when they made a little party and went out to this lighthouse. Here is Lord Rayleigh treading up the lighthouse steps and looking at the boats, and here they all are at lunch.

Now then, I have a collection of material that I have identified thus far only as unpublished. Either the derivation wasn't leading anyplace or else he wasn't happy with it. He made a start at some of the material and then did not continue it any further.

Well, that concludes the description of most of the major acquisitions of our Archives. A few months ago I went over again to show the family how my work was shaping up. They showed me one further remarkable treasure — the room at Terling where they keep family papers. Quite a bit of this was Rayleigh's scientific correspondence. Here were dozens of little boxes, jammed full of little papers and letters. "J. W. Lord Rayleigh's letters and papers relating to the discovery of argon, not including correspondence with Ramsay." Another "Correspondence with Ramsay," and another one, his "Rough calculations for Argon." Now these items the family is retaining, but they have said that for our Archives we can borrow them, photocopy them, and then return them. And I might say that I got thrown for a loop and a half the other day when Charles Strutt arrived at the airport with an extra little treasure chest — a suitcase of some additional correspondence that I had not seen prior to this time, and I had thought I had a complete index of the correspondence in that room.

But this suitcase, I have only spent a couple of hours looking at it so far so I hesitate to tell you very much about it. But it seems to contain almost all of his scientific correspondence—every letter he received from 1870 until 1919 in three bundles: one set prior to the Cavendish professorship; one cluster that is Cavendish; and one that seems to be post-Cavendish. Each set is alphabetically sub-divided.

Well, I sat there and I sorted out immediately 17 Michelson letters and one Morley. These I brought here today because I knew Professor Shankland would be very interested in them. I found a very fascinating Planck letter that we were puzzling over just before the ceremony. There is a huge packet of R. W. Wood letters, and letters from many people who I never heard of. It is a remarkable collection.

I estimate that there are at least 1000 letters in the suitcase, which we will copy and index and then return. But I am not really prepared to say what we have in the collection of letters, except that it is going to add a great depth to our Archives.

# ADDRESS BY THE HONORABLE CHARLES R. STRUTT

DR. HOWARD: I would like to ask our honored guest, the representative of Lord Rayleigh, the present Lord Rayleigh's brother and the son of the fourth Rayleigh, if he has any remarks he would like to make. Mr. Strutt.

MR. CHARLES R. STRUTT: Well gentlemen, obviously I do want to say something and that something, first of all, must be what a memorable occasion this is for me and how grateful I am. I think I must thank the United States government for inviting me to be present, but governments are rather impersonal things and my real thanks are to Dr. John Howard who, from beginning to end, has been a great friend. I think I would like to tell you a little bit about it from my angle. You have heard it from his.

As soon as I heard that he was nosing around looking for further material, I thought to myself—now this is my opportunity. There was something very much on my mind and it was this: my father died comparatively young—he was only 72—but I think he must have known that his time was getting a bit short. At any rate, for the last year or two of his life, his chief objective was to gather together his scientific papers that had been published in various periodicals and scientific journals. He wanted to get these together in one volume with some notes and comments of his own, and this work was about 95 percent finished, I should say. And once he said to me: "I think you will write something about me after I am gone," and this was a kind of, you know, Hamlet's father sort of remark, and I felt that his ghost was haunting me because this had never been done.

His collective works had never been put together. We tried ineffectively to get a scientist who had the time and knowledge and inclination to do the last steps and to see the thing through the press. We failed to find such a person in England, and it never occurred to me to look elsewhere. So, anyhow, when John Howard appeared on the horizon and I realized that he had a good deal of enthusiasm, I thought to myself—well we have got some more material and that is the carrot—but he's got to help us to finalize these collected papers of my father. And I put this to him pretty quickly and with no trouble, I am glad to say, and the financial part of the whole thing went through like a knife through butter.

It was a most gentlemanly affair, I think you will agree, and he promised to do this, and I believe that this is now getting very near to finality and he has been responsible for finishing this off. And I hope to see it through the press; I rather hope to see the final result in the very near future. That is that part of it and perhaps I might also say that I think I speak for my whole family when I say how we are delighted that it was in fact the fourth Baron's work—our father's work—that got all this on the move.

I recognize, of course, that he was a lesser man than my grandfather, but I have always felt that my grandfather's fame was sure and secure whereas my father's—well he was in a shadow—but it was his work and the interest of this organization in his airglow work that triggered the whole thing off. And that personally gives me intense pleasure and satisfaction, because I was very close to my father and I loved him very much. I feel that if he were here today that he would feel completely happy that this has happened.

Now my own part in all this. I can give you a little further background. I don't want to take up much of your time, but these manuscripts of my grandfather, not the notebooks, but the others, I had some little hand in their preservation in this way. One day, I think it must have been about 1935 or 1936, my father said to me: "I think I am going to get rid of grandpapa's papers." He said, "I think there is nothing further to be extracted from them. There are some unpublished papers, but I showed them all to Sir Joseph Larmor and he recommended two or three for publication, and I don't want to dilute his work or reduce the standard of his work. What he didn't think worth putting into print, I would prefer should be left in oblivion." So he said, "I now think there is nothing further to be extracted from them and I am proposing to destroy them."

Well, I thought this was not so much a statement as a question to me as to whether I approved, because I was considered to be the family archivist and I disapproved and I said, "Don't do that, it may be there's no interest in these things now, but in 100 years there may be"; and he said, "well, they clutter up my room and I don't want to see them around any more." Well, I said, "I'll take them away and you shan't see them any more." And I took those three boxes and I hid them

away thinking, you know, that there would be no further interest in the material in my time. Then when John Howard arrived at Terling and asked, in an innocent sort of way, "if there was anything further?", I went and roused them out and he did seem quite interested. So I did play that small part in their preservation.

Now, one thing I would like to say is this: that I think, naturally, if some English library or institution had shown the remotest interest, I should have been quite pleased if these things had remained in England; but quite honestly, when my father died it would have been open to Cambridge University, or Trinity College, Cambridge, or the Royal Institution or the Royal Society to write in and say what is there—can we have it? and I think it would have been given to them as a gift. I don't think there would have been any question of selling anything.

But they didn't show the slightest interest and the Air Force Cambridge Research Laboratories came along—showed very great interest, and I think that everything shows—this issue of Applied Optics, etc., shows how they have, in fact, rescued the names of my father and grandfather—I won't say from obscurity, but at any rate they have revived interest in England and I speak for my brother and certainly for myself when I say that I am delighted that they should be here and I have no feeling whatever that they should be in England. They should be where they are most appriciated—is my thought.

Now, that really is virtually all I have to say, except this: we still live in the family home, my brother lives there. I live about 2 miles away. It is very accessible from London; it takes about 40 minutes on the train, and if anyone here finds himself in London with a spare day and would like to come down and look around, my wife and I would be delighted.

I hope you won't all come on the same day, but we have a fairly big house ourselves and we should be delighted to do whatever we can for you and certainly to give you lunch in the middle of the day and take you around. Give us a little notice if you can, but if you can't give us any notice, ring up and come.

Thank you very much.

DR. HOWARD: I should also point out that Mrs. Strutt is here. So it is Ladies and Gentlemen this afternoon, although she is not really staying for the afternoon discussions, but we are very honored to have her here too.

We are glad that you saved those papers, because although it is true that one could say that Rayleigh's essential work was published, I think the historian of science finds it of some interest how things were published and what paragraphs were left out at the last minute and the process is of some interest to him. And certainly the correspondence—the way the material grows—this is of great interest.

# DISCUSSION OF THE SCIENTIFIC WORK OF THE THIRD BARON RAYLEIGH

DR. HOWARD: Now, we want to do some discussion where more of you are involved, and I would propose to try first a little while of discussion of John William—starting more or less chronologically with his activities. His first activities I don't think are going to ring any bells; his earliest interest was photography. His first paper, I believe, was in mathematics. He was early in the mathematical society, but his first pay dirt, you might say, was the "Theory of Scattering." But even before we get into that, I would like to mention one thing.

You know that one of the first things that Robert did in 1924, soon after he became the fourth Lord Rayleigh, was to write a biography of his father, "The Life of John William Strutt, the third Lord Rayleigh," and this was published in 1924. And I have here a rather interesting review of it from ISIS, The Journal of the History of Science, of 1924, which in many ways, I think, is singularly unperceptive in that the reviewer says a lot of things that are ture, but his capability of looking forward went a little bit wrong: "John William Strutt, the third Baron Rayleigh, was one of the greatest physicists of all time. He was an amateur, unhampered in the full development of his powers. His biographer is his son, himself distinguished in the same field, lacking neither knowledge, nor sympathy, nor literary skill." And yet, the reviewer mentions earlier that the shortcoming of the book is that you can write a biography of a politician or a statesman because we are interested in the events that these people were associated with, but it is very difficult to write a long biography or a glamorous treatment of a scientist because scientific writings just don't blend to glamour. They are impersonal facts you

might say. "Scientific events are recorded fully and once and for all in technical publications, and though the personality of the scientist is associated with his work as closely as is that of the artist, his influence is seldom apparent outside of the laboratory."

But now, the shortcoming of John William according to this reviewer, was that "The vigor and deep insight that marked Rayleigh's smallest contribution to physics seems to have exhausted themselves in his work. When he left it, he became a typical English squire with the best qualities of his class, its dignity and sense of responsibility, but also many of its defects, its lack of artistic perception and of imaginative sympathy. He was a model landlord and head of a family, but his opinions or lack of them on politics, religion, or art were indistinguishable from those of many with no pretense to intellectual eminence. He was a professor, but he founded no school and left no pupils. He filled high offices, but made little mark on them. Those who came into contact with him often fell under the spell of his gentle charm and kindly courtesy, but for most of the world all of him that matters is contained in his six volumes of collected works."

Well, in many ways that is true, because his main influence is in these six volumes of collected works. But where the reviewer really went wrong, I think, is in writing this in 1924, because in 1924 and 1925 along came wave mechanics and quantum mechanics, and the first thing that all of these people needed was a tool to study quantized wave effects and they dipped right into the "Theory of Sound"—into Lord Rayleigh—and you find the Rayleigh-Born approximation, the Rayleigh-Ritz method, the Schrödinger-Rayleigh method, and even the Wentzel-Kramers-Brillouin method, which is Rayleigh again. Rayleigh moved to a very pre-eminent spot; it was the wrong time to write the review. This, of course, doesn't give him personal glamour, I suppose.

Another thing that makes that review a little wrong, I think, is that Rayleigh, of all the great physicists, of all the big pioneers, of the last 100 years or so, is one that can still stand very much on his own feet in terms of references to his own original work. If you look in any journal today you can still find references right back to Rayleigh. This is not really true of Kelvin or maybe even Einstein. They establish laws and theories, but you don't reference them anymore. You very seldom see a reference to Maxwell directly. I don't, maybe Victor Twersky does. Last year, for example, I looked Rayleigh up in a citation index; it lists who was referenced in the standard physics journals. I counted some 65 references to Rayleigh, and that is more than most living people get. You just don't find that for the other great big people. It shows that he worte his work in a more or less timeless way. I think his work is more than just an ordinary memorial of collected volumes.

That concludes my remarks. I would now like to open the discussion. I think the first aspect of Rayleigh where we might get some informal remarks would be Rayleigh scattering. We have several good Rayleigh scattering people here, and

even Dr. Penndorf is a Mie-scattering man—you might count him on the edge of a Rayleigh scattering group. We accept him. But Dr. Victor Twersky, I wonder if you would at least preside at this little section of our discussion?

## Scattering Studies

DR. VICTOR TWERSKY: Thanks John. If the rest of the speakers this afternoon were informed as late as I on this matter, it isn't going to be too formal a session. John told me about this just when we left for lunch. But I'll amplify on a few of the points that he made.

To begin with, John mentioned that Rayleigh had 150 papers on optics and about 140 papers on acoustics. That means roughly 300 papers that at least touch on scattering. If you add in all of the work that Rayleigh did on potential problems, as the low-frequency limit of the wave problems, you could say that about 400 of Rayleigh's papers out of the 440 involve scattering or problems of the wave equations in one form or another.

When I reviewed Rayleigh's work in scattering (its broad lines) in the special issue of Applied Optics, I did not cover all 400 papers—only about 100 or so— and the term "Rayleigh scattering" was used facetiously in the title. The main point that I made, and I will reiterate it here, is that every single line—main line— in scattering theory was essentially either originated or developed explicitly by Rayleigh. (There were some things that had been done by Stokes and by others that Rayleigh picked up and made more explicit.)

This includes everything from scattering by small objects to the inverse 4th power wavelength dependence (which to many people means exclusively Rayleigh scattering— but that was merely one class of problems he considered) on up. The so-called Born approximations—Rayleigh introduced these, and not just the first. He also computed the second Born approximation for a sphere in one of his early papers (No. 74; 1881), and discussed the complete iterative procedure. (The WKB procedure—John to the contrary—is not Rayleigh's; it is Liouville's, but that is another story.)

Rayleigh not only obtained approximations and series solutions for isolated scatterers, but also for periodic structures, gratings, rough surfaces, and random media. Every single class of scattering problems that we consider today was essentially either introduced or made more explicit by Rayleigh. Now, there is no point in elaborating on this for the present group, because this theme is discussed in detail in my Applied Optics review. Instead, I'll follow a thought that arose earlier.

While sitting and looking at that austere picture of Rayleigh on the wall, I was reminded of a couple of paragraphs by Heaviside in his book "Electromagnetic Theory." You know Heaviside would digress in his mathematical work (with polemics on quaternions, etc.), and he has a section on the name "physicist" or "scientist." He preferred the name "natural philosopher." He describes the image that the term "natural philosopher" conjured up for him, and it is identical with the image of Rayleigh: the sideburns, the dignity, and everything else. And certainly the image that I had of Rayleigh as a student, through the papers and things that I read, was very much along those lines. But in preparing the review for John, I got quite clinically involved with Rayleigh, and various matters came up that showed him to be a more vital and more interesting person than I had imagined him to be.

To begin with, I turned every page in his six volumes (because I did not really know if I knew all the papers which were on scattering), and I did the same for the "Theory of Sound." If you have done this, you know that there are some papers which Rayleigh simply abstracts in his collected works, and says that the material was picked up in his "Theory of Sound"; when you get carried away, you go back to the original papers and see what is going on. There are also cases where Rayleigh is somewhat abrupt with another's work. I mentioned some of these things parenthetically in the review, and I'll touch on some now because they show that Rayleigh was a very human man and had some interesting—well, I don't know the details except for those pieces that actually show up in the papers.

As an example, Rayleigh's very important paper (No. 14; 1872) on scattering by a sphere—the scalar separations of variable solution—is not reprinted in his collected works. There is a half-page abstract and the reader is referred to the "Theory of Sound," but the book does not quite cover all of the original paper. There are several sections that are omitted, and there is one in particular that is noteworthy. Starting with the complete solution for scattering by a sphere (as you know, there are ways of getting different approximations—high frequency, low frequency, etc.), there is one method where you replace all the Hankel functions by their high-frequency asymptotic forms and come out with a very simple result for the scattered field. The field on the sphere is then proportional to 1 minus $\cos\theta$, and the only essential point is that this predicts a zero value in the shadow and the incident value times a factor of 2 on the "lit side." Rayleigh had done this heuristically in the original paper, but the material is not reprinted in the "Theory of Sound." Then about 1903, Macdonald had a paper on scattering by a large sphere in which he considers this asymptotic procedure for getting such an approximation, and Rayleigh (No. 287; 1903) follows this with a very nice note pointing out why it is incorrect and what one actually has to do to get the right kind of results. The interesting point here is that Rayleigh does not refer to his own original treatment going back about 20 years.

I think the most amusing such case among all the papers arose in connection with Love. There was clearly no love between them on another score: Love had attacked Stokes (and indirectly Rayleigh) on some analytical work, and Rayleigh (No. 296; 1904) had countered in print. But the incident I have in mind particularly relates to a paper, I think in 1899, in which Love considered scattering by a dielectric sphere (on the so-called "Mie solution" that Logan has pointed out goes back much earlier). Love had gotten the electromagnetic solution and approximations—expansions— in 1899. In 1910, Rayleigh (No. 344; 1910) started with Love's exact result and pointed out that there was something wrong with the final approximation; then he gave a very detailed treatment of the problem and it turned out that in two of the terms of the expansion that Love had developed, a factor 2n + 1 should have been 2n + 3. The most interesting thing about the Rayleigh paper is that at the very end of it he states that Professor Love wrote him and this error in the 1899 paper of Love's was corrected by Love himself in the next volume of the journal.

I just mention these matters to try to get away from the very austere picture; by going through the papers and reading some of the parallel ones of that time, one gets a much more rounded picture of Rayleigh as an individual. John mentioned the son's biography. I have felt that the son leaned over backwards to keep out—well—human problems that Rayleigh was involved with at the time. These would have added greatly to the interest of others working in the same subjects. Now Mr. Strutt, John also mentioned that your father had left an annotated copy of the biography with more personal details. I think many of us would like to see that in print.

Well, I don't know what I am supposed to do here. I just mentioned these points to get things rolling. What are we supposed to do, John?

DR. HOWARD: Anyone have any comments relating to scattering in general?

PROFESSOR SYDNEY CHAPMAN: You mean to say that Love himself corrected this the year after it was published?

DR. VICTOR TWERSKY: If I refer to this copy of the Rayleigh issue of Applied Optics, I can tell you exactly what I mean. (One shouldn't be asked to re-read one's work within a year or two after it was written, but I did look at this earlier.) It was corrected in the next volume—yes, here is the way that I say it in the review in a paragraph (p. 1153) that starts: "In a note Rayleigh refers to Love's 1899 results for scattering by a dielectric sphere" and so on and so on. Now at the end of the paragraph I say: "At the end of this 1910 paper, Rayleigh adds a

note that he had recently heard from Love that the error in Love's 1899 paper in Volume 30 of the Math Society Proceedings had been corrected by Love in Volume 31. Thus was Love's labor lost."

DR. HOWARD: There is one other. When you say the--what we call the WKB method—Wentzel-Kramers-Brillouin method, Wentzel published this in 1924 in a Dutch journal. This had also essentially been given in an early Rayleigh article of 1901 and 1902, and parts of it are on the "Theory of Sound" of 1879; but it is true that you can go back to Liouville for the same treatment. There is another example of anticipation of "Rayleigh scattering" that has been pointed out by Nelson Logan of Lockheed Aircraft Company, who unfortunately could not be here today. Although Logan is a very devout Rayleigh enthusiast, he has published a fine review paper showing that the essential mathematics of what we now call Rayleigh scattering had been published by Clebsch in 1869 in Crelle's Annalen. But Rayleigh didn't read German and it was published in a different form for a different problem. It wasn't applied to the atmosphere.

DR. RUDOLF PENNDORF: Oh, but it was! I read Clebsch's paper after Logan pointed it out and this I must say, although I read it more than 10 years ago so I may not remember it exactly. Clebsch was a mathematician somewhere in southwest Germany—I think one of the universities. A friend of his suggested a problem to him: namely, in geodesy they had a problem of perfection in the air to get exact measurements for the theory of light, and he said, "There must be some influence by particles." Clebsch attempted to compute essentially the scattering by particles.

This was before the Maxwell theory came out, so the speed of light is not in it, and this causes all kinds of problems, but mathematical solutions of Bessel functions and Legendre polynomials are already in Clebsch's paper. But his paper ends like a lot of mathematical papers sometime end. There is no numerical value in it. He develops the mathematics and then he sees he cannot solve the problem so he quits at that time.

If you look through Rayleigh's papers, he always, at the end of the ones I have read, tries a numerical solution so that even the applied people who are not solely interested in some mathematical derivation get useful results, even if they only are interested in the final formula and not in all the derivation. I would call the Clebsch paper a classical mathematical paper. He only mentions why he starts out with that problem at all, but he is not at that time able to bring it to a solution; therefore, his friend who suggested the problem to him can never really use the result.

DR. VICTOR TWERSKY: I can mention how I understand the situation, but I have not tried to read the original paper. Clebsch was a brilliant applied mathematician. (This is the Clebsch of the Clebsch-Gordan coefficients.) He considered scattering of an elastic wave by a sphere, and worked with vector equations. It was before Maxwell's Equations, but you know half of Rayleigh's work (even after Maxwell's) was done without Maxwell's Equations. (What I say now is just a footnote: Rayleigh in one of his papers (No. 247; 1894), when he first started to use Maxwell's theory, says that it was "To be preferred on every ground except easy intelligibility"; he just didn't feel at home with it.

Of course, as you know, Kelvin never could see Maxwell's Equations; he almost did in the preface that he wrote to Hertz's collected papers. Many scientists came too late for Maxwell's Equations. But in any case Clebsch looked at scattering, and used a vector formalism as used in electromagnetics today. It doesn't really matter that he didn't know about the present explanation of his physical parameters. As I remember Logan's article, the problem Clebsch was interested in was high-frequency scattering, and he probably got entangled just (as I mentioned earlier) the way that Rayleigh and Macdonald had at one stage. They were trying to work with asymptotic expansions of Bessel functions, using what we now call the "Sommerfeld representation"; the correct forms you need are now called the "Debye forms," and Debye developed them for this problem. If you just plug the Sommerfeld forms into all the coefficients that come up, you get some ridiculous results. So Clebsch had the formalism, but did not come out with anything of significance to applications. Logan has done a fantastic job in tracing back the history.

DR. HOWARD: Logan's paper, by the way, is in the last August Proceedings of the IEEE, Volume 53, August 1965. I have three or four copies of it here.

DR. RUDOLF PENNDORF: There was also an old AFCRL report by Logan.

DR. HOWARD: Yes. Logan, by the way, although he couldn't make it here, wanted to say—in fact, he put a footnote here. "This work was done because"-- it was begun because he was at Air Force Cambridge and had access to our splendid library where he could find Clebsch's paper and such things. Dr. Barakat?

DR. RICHARD BARAKAT: I don't think there is any controversy because this problem was resolved many years ago in a book by Todhunter and Pearson called,

"The History of the Theory of Elasticity." In it, in the second volume, Part 1, there is a discussion of Clebsch's paper, and Pearson, who was the successor to Todhunter in completing this work, explains Clebsch's work. Therefore, I think a large number of people are under the impression that there was never any controversy. I don't think that anybody who has tried to read Clebsch's paper has ever been able to make any sense out of it, even though Clebsch was a top-notch mathematician.

DR. HOWARD: Rayleigh didn't begin to study German until 1870 or so: he just wasn't reading Clebsch.

DR. VICTOR TWERSKY: But—Rayleigh and Clebsch? Never together! Logan referred to Clebsch and Mie (M-i-e, not me). Rayleigh was the first to consider scattering by spheres as a scalar problem, as well as scattering by cylinders. He solved the scalar problems by separations of variables, published those results, and treated them in detail. As for the vector problem—the electromagretic problem of the sphere where one needs a vector formalism—Rayleigh initially considered limiting cases which he could do by other methods (essentially Green's function methods, as we know them today) that lead into the Rayleigh-Born approximations for general shapes, perturbatior approximations based on the solutions of potential problems, etc. But you see, Rayleigh didn't consider deriving the series for the vector problem of scattering by a sphere.

He didn't need to because, even if he didn't know of Clebsch's work, he knew that Love had considered scattering by the dielectric sphere, and that Thomson had considered scattering by a perfectly conducting sphere. Love's work was about 1899, and Thomson's was in 1893 for the metal case: they had the Mie solution in essentially the long-component form—it is a question of form.

Love and Thomson worked, I think, in Cartesian coordinates (I better check that)*, so you have to work harder to get some of the things that follow more directly from the Mie-type vector representation in spherical coordinates. That we have known for sometime—that Love and Thomson had gotten the series solution for the electromagnetic problem of the sphere, and Rayleigh was not involved in that derivation at all.

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*Professor Chapman comments that Lamb also worked in Cartesian coordinates. Lamb once told Chapman that he thought vectors were only useful as shorthand: he did not know that they were also useful as a tool in working things out.

DR. HOWARD: If you look in the very first paper, Paper 8 on scattering, he does say he is considering an ether that is loaded with inertias of these little particles. He says that the formalism of the mathematics is completely independent of this assumption. And 10 years later he came back and redid it with Maxwell's equations. Roughly every 10 years he returned and re-examined scattering and added something new. In fact, his paper on scattering by dielectric spheres was as late as 1914.

DR. VICTOR TWERSKY: The essential feature is that the scatterers that Rayleigh considered in that Paper 8 were small compared to wavelength, and this is why he comes out with the one over $\lambda$ to the 4th power dependence. Incidentally, the most interesting part of that paper, aside from the scattering part, is the appendix which leads one into Stokes' big paper on diffraction; that's what the ether model of 8 is based on. This paper by Stokes, I think it was written about 10 years (actually 22) before Rayleigh's, gets the Green's function for the dipole in such an elastic-ether model for the electromagnetic field. So what Rayleigh had to do was to consider scattering with the Green's function that Stokes had introduced. Since he knew what happens to the radiation from a dipole source in such elastic media, Rayleigh could concentrate on getting the all-important scattering coefficient.

DR. HOWARD: So you think we are still on fairly safe ground to call this Rayleigh scattering?

DR. VICTOR TWERSKY: Sure. The whole thing.

DR. HOWARD: Well, that is an interesting discussion, but if we are going to try to cover all Rayleigh, we are going to have to move to other topics.

You know, the next thing that happened to Rayleigh was that he got sick with rheumatic fever and was advised by his physician to recuperate in the Mediterranean climate. He spent three months drifting along the Nile River in a houseboat, and because he didn't want to stifle himself, but wanted to keep intellectually active, he wrote the "Theory of Sound" or at least the first volume of the "Theory of Sound," which is the philosophic volume and doesn't have the literature references and such. When he came back, he added the second volume and did the literary work and documentation he needed in the library.

I would now like to ask Dr. Lindsay if he would like to make any comments on the "Theory of Sound" and Rayleigh. Dr. Lindsay wrote the historical introduction to the 1940 Dover edition of the "Theory of Sound," and now 25 more years have flowed under the bridge—just an analogy—down the Nile.

DR. BRUCE LINDSAY: Well, after listening to the high physics of Victor Twersky, my remarks will probably seem rather mundane and trivial. Of course, that is what many modern physicists think about acoustics anyway. But as a matter of fact, Rayleigh did it all, knew it all, and, therefore, there wasn't anything left for the rest; acoustics then became a branch of engineering or possibly physiology and there was nothing left for a physicist to do. But, of course, this is a distortion and we find evidence for that in Rayleigh's work in sound.

The "Theory of Sound," the great bible if you want to call it that of acoustics, was indeed a very great summary of acoustical results which had been obtained before both by Rayleigh and by many other people. It is rather interesting indeed, I think, to know that Rayleigh's own interest in this subject, according to what he said himself, came from his desire to know more about it because of his reading of Helmholtz's famous treatise on the sensation of tone which had come out not long before his own work. However, in the "Theory of Sound," as has been emphasized by Victor Twersky, he did introduce new methods and new ideas of techniques which have proved of enormous value and have been very fruitful in modern physics. Somehow he seemed to have the knack of seizing upon ideas which would be valuable even if he himself did not follow them up in detail.

Perhaps I might give an illustration which relates to modern acoustics and that is the suggestion made in his paper of 1899 which came out after the revision made on the "Theory of Sound," I believe in 1894. In this 1899 paper on the cooling of air by radiation conduction and the propagation of sound, he commented on the difficulty people were having at that time to explain the absorption of sound in air. By using ordinary transport processes, that is viscosity, and heat conduction, experimental results were always greater than the theoretical results. Of course, people had to try to think what the explanation might be. Many people thought it was simply atmospheric conditions and, of course, wind and temperature gradients and all this would have effects, even if one has perfectly quiescent air—still the absorption is greater than would be classically predicted by Stokes and Kirchhoff.

Rayleigh at that time made the suggestion in a simple paragraph, but did not follow it. He suggested that we might ultimately find the reason for this anomalous attenuation of sound in air in intermolecular energy exchange in which the translational energy of the molecules, which is primarily responsible for the propagation

of sound, is transformed into internal modes of energy of molecules, vibrational and rotational states.

As I say, he didn't follow this up, but this was the suggestion which later led to the work of Einstein, Herzfeld, Knudsen, Case, and all the people who founded an entirely new branch of physical acoustics—molecular acoustics—which, of course, in turn had a great deal to do with molecular physics and chemistry. This is just an indication of how there was this uncanny insight into a very fundamental problem which formed the basis for an entirely new development in physics.

Rayleigh, of course, obviously knew a lot about the propagation of sound, the emission and radiation of sound from sources. Unfortunately, much of that early work could not be tested at the time simply because adequate instrumentation was not available. We had no ultrasonic sources. Rayleigh, I believe, had at his disposal the bird whistle; this was the extent of ultrasonics in his time. Later, of course, indeed using some of the very concepts that Rayleigh himself suggested with respect to electromechanical ideas, new ideas came up and ultrasonics became a fact and many of these results of Rayleigh could then be actually experimentally checked.

I don't think it is necessary for me to go into great detail and mention many of the examples which have led to important results in acoustics of a very practical character. I will mention one again, namely the introduction of the reciprocity idea. Now it is true that this went back to Helmholtz, but Rayleigh generalized it, criticized it, and made it into a powerful tool. Today it is standard technique in the calibration of transducers. This is a very interesting example of the development of an important practical application from a highly theoretical idea.

Well, I think one could amplify this at great length. I won't take the time to do it. Perhaps I will close merely with an anecdote that I remember from the celebrated biography in which it was said that often when the family observed Lord Rayleigh intent, very intent, on reading something and they looked closer, they usually found he was reading one of his own papers. I am sure that we all agree that he could not have found more appropriate subject matter.

DR. HOWARD: I might make a little postscript on the bird whistle, his source of ultrasonics. After they had had a little fire in the attic at Terling in 1930, Robert, the fourth Rayleigh, got a little worried that all this priceless stuff could have been destroyed, so he took quite a few pieces of apparatus and gave them to the Science Museum. One of them was a bird whistle.

I said earlier that Rayleigh liked to write on the other side of pieces of paper because he didn't like to spend money on paper; I might add that he also didn't waste money on equipment either. This particular bird whistle was made out of

an old coffee can and it says on it: "This is sold as a mixture of coffee and chicory." He never believed in having anything constructed or built, if he could form it himself out of home-made products. In fact, one time J. J. Thomson visited the laboratory in the 1890's and looked at the way this place was fastened together—lenses stuck on the tables with sealing wax and things like that—and he said: "It looks like the whole place is put together with sealing wax and string"—which has become a famous expression. The very first issue of Physics Today had sealing wax and string on the cover. Isn't that right—1945 or so? As I recall, Oppenheimer's hat was sitting on the table along with some sealing wax and string.

Well, I think Dr. Lindsay has given a very nice summary of Rayleigh and acoustics. As I said earlier, the acoustic people still claim Rayleigh was primarily an acoustics man. If you throw the "Theory of Sound" in with the fraction of his papers that was acoustics, you could tip the balance that way.

MR. CHARLES R. STRUTT: Could I just say something?

DR. HOWARD: All right.

MR. CHARLES R. STRUTT: It's personal. I am not sure it comes in the biography, but in fact when my grandfather and grandmother were first getting together, I rather think, indeed I am sure, that some of the excuses they had for tete-a-tetes, which were rather difficult in Victorian times, was that she was teaching him to learn German. She had had a German governess and she knew German and he didn't, and also she was quite a good musician and they got together over Helmholtz's Tonempfindungen. He gave her a copy of Helmholtz and so on, and this also cemented matters. Had it not been for that, perhaps I wouldn't be here.

## Electrical Studies

DR. HOWARD: Victor Borge closes his program by saying: "I would like to thank my mother and father who made this program possible, and my wife who made this program necessary."

But the next major activity I would say was the Cavendish professorship, 1880 to 1885, and his very intense studies of electrical units and electricity. This period was also the high point of creativity for Rayleigh. He published 60 papers during these 5 years, which is one per month on the average. A little

better than the rest of his life when he slipped down to only 10 per year. But most of these were the fundamental papers on electrical units. And, as you may or may not know, when we talk of a volt, an ohm, or an ampere and we talk in practical terms, the papers of Rayleigh and his sister-in-law, Mrs. Sidgwick—those papers on the absolute determination of the ohm of '881, the volt, and the electro-chemical equivalent of silver—are still the basis of the legal definitions in the United States, in the world.

The international volt-ohm-ampere are still the ones that Rayleigh published in those days. But let us turn a little bit to his other contributions in electricity, electronics, and such things. You don't usually think of him in those fields because one thing Rayleigh did not have at Terling was alternating current: he didn't have electricity. So, it was a little hard for him to do experiments in that area. In fact, this was the reason he was so willing later to become the professor at the Royal Institution. He occupied this chair of experimental physics because there was alternating current in London. At Terling, he had to use Grove cells and Clark cells and things of that sort.

Another man who wanted to come today and could not make it at the last minute, was James Wait at the Bureau of Standards, who is Editor of Radio Science, a publication of the Bureau of Standards. In a recent issue, September of 1965, Dr. Wait wrote a very nice article chastising the people writing in electronics for not really going back and documenting their sources to Lord Rayleigh when they should, and he gives a long list of Rayleigh's works that are germane to these. It is an editorial comment on the'Scientific Papers." He really was inspired to write it when this appeared on the scene, but he goes through many of Rayleigh's contributions that have appeared in electronics.

I believe it was Rayleigh first who worked out the coupling of circuits. Karl Willenbrock once told me the anecdote that there was a time period in the middle 30's when every student's thesis began with the sentence: "In 189X Lord Rayleigh did so and so" and then one continues with the thesis. Here is one in November 1965, "Archives for Rational Mechanics and Analysis," a paper on Dirichlet's problem for the Helmholtz equations. First sentence: "In 1897, Lord Rayleigh, reference 16, considered the relationship" and then he continues the paper. But that sort of beginning was very characteristic of an entire generation of papers. Dr. Willenbrock, do you have any comments of Rayleigh in electronics that come to your mind?

DR. KARL WILLENBROCK: Not particularly. I think there is just one item: you mention the unit problems and this is one that is still being fought out as you know and people are very interested in this. Just recently we ran an article in

Spectrum, which is the Civil Electric-Electronic Engineers' major journal these days, in which a survey was made of the unit question and it traced it right back to Rayleigh's contributions. I think this just came out in March. So, we are right in the spirit of things here.

DR. HOWARD: Good. That is right. The entire March issue of Spectrum had several papers such as "the IEEE Takes a Stand on Units." Professor Sandy Brown, your contribution in plasma physics is close to electronics, so I wonder if you have any remarks.

PROFESSOR SANBORN BROWN: Well, all right. I, however, want to use a little of these papers which I never saw before.

DR. HOWARD: And I hope they are still going to be there. Tomorrow we are going to have an inventory.

PROFESSOR SANBORN BROWN: Actually, of course, the general public has been worring about plasmas for long enough to make it very popular, but one of the things that always turns up now in the television and the radio is the astronauts as they are trying to get back through the ionosphere and the fact that they lose contact with their communications because the density of electrons which are produced by the capsule coming down, produces enough ionization so that none of the radios, none of the tracking on the radars work. And I think it is worthwhile, just for a minute, to point out that the original calculation of this plasma frequency was done by Lord Rayleigh in 1906. I thought it might be worthwhile just reading from this paper for just a minute. Actually, this is just a personal desire of mine. One of the nicest things I remember is when I gave a Friday evening discourse in the Royal Institution a few years ago and had a chance to use some of the original equipment of Rumford during that lecture, and I sort of want to talk about Lord Rayleigh with his paper in front of me here.

The problem which he was trying to address himself to was the emission spectrum of the hydrogen atom. This was in the days when J. J. Thomson's pumpkin atom was the current thing to worry about. That is, the atom consisted like a pumpkin with seeds in it, where the seeds were the electrons and there was a positive jelly. The thing that Lord Rayleigh tried to determine was whether he could calculate the spectral emission from the harmonics of this jelly—positive-negative jelly—which was oscillating. This is the paper entitled "On the Electrical

Vibrations on the Constitution in the Atom." "In illustration," I am now reading from this, "In illustration from the view suggested by Lord Kelvin that an atom may be represented by a number of negative electrons or negatively charged corpuscles enclosed in a sphere of uniform positive electrofication, Professor J. J. Thomson has given some valuable (first of all he said "interesting," I see), calculations. Some of the most interesting of Profe. sor Thomson's results depend essentially upon the finiteness of the number o. the electrons and since the experimental evidence requires that in ny case the number should be very large, I have thought it worthwhile to conside hat becomes of the theory when the number is infinite." He then supposes that there are equal numbers of electrons and ions and proceeds to displace the elec rons in a sphere and arrives after a little mathematics at the (may I use the blackboard?) equation:

$$P^2 = 2\pi\gamma\rho_o$$

where $P^2$ is the square of the frequency and gamma is $e^2/m$, $\rho_o$ is the charge density.

In other words, this is the correct equation that we all use in case of a sphere. We usually do it for infinite media, but for a sphere, this is the solution which Rayleigh comes out with. To my knowledge, this is the first calculation of the plasma frequency. The next time it was referred to was by Langmuir in 1925 when he was worrying about his problem. The same equation applied specifically to plasma instead of the Thomson atom. That was, I think, worth pointing out.

DR. HOWARD: I think as far as the ionosphere is concerned, credit for this is usually given to Sir Edward Appleton who died last year. But is Nate Gerson here? He was going to be here. He wanted to point out that his principal activity these days in ionospheric research has been the study from the antipodal points of events that distort the ionosphere, like a nuclear burst going off or something of that sort. And that in order to calculate the transmission of these characteristics through the ionosphere, he has used Rayleigh's theory of the whispering gallery: it is the same mathematics as the whispering gallery. He just wanted to get up and say that he was still very much using Rayleigh's theory for his present day work.

DR. RICHARD BARAKAT: I would like to mention one thing that people seem to feel about Rayleigh. Most people are too compartmented: I mean acoustics people reading acoustics, people who are in optics, looking only at optics, and so on. I had a rare, rare privilege myself of being self-educated to some extent, in having

a professor once, who was a student of Harry Bateman, and who brought me up on tripos-type mathematics and physics. And I am probably the only man in the world who ever read Lord Rayleigh's papers as part of a course, and I probably am the only man in the world ever to take a course in the theory of sound by reading Lord Rayleigh. More of us should take the attitude that Lord Rayleigh's books are really applicable to many problems. Anybody who works in acoustics can immediately see that Rayleigh's results are applicable to many other areas, and perhaps Rayleigh realized himself that waves are waves. I think it is the general point of view rather than the specific problem he took.

I remember some years ago when I was a student at M.I.T., looking through the stacks of the Bell Telephone System Journal, where you will find the most beautiful review of Lord Rayleigh's "Theory of Sound" by an electrical engineer in 1927, when Rayleigh's papers were republished. This review was by Foster and if any of you can get hold of it, you ought to read it: a really magnificent review of this book by a very distinguished electrical engineer. You will be quite surprised at the influence Rayleigh has had in the general sense without focusing on any particular problems.

## Argon and Other Studies

DR. HOWARD: Dr. Price, would you like to have the chair?

DR. DEREK J. de SOLLA PRICE: I don't know whether it fits, but I'd like to perhaps sweeten the already sweet occasion to John by owning up. They say that success, especially academic, is always rather nicer when it is attended by the failure of another colleague. I have to own up with a small detail.

About 15 years ago, I was acting as historian to the Cavendish Laboratory and had collected the museum and the archives of papers of everybody but Rayleigh. All the Maxwell material, the J.J. Thomson and the Rutherford, but didn't quite have the problems John had in going through Embassies and Consulates. We knew that the papers were there in Terling and I went down. The Strutts were hospitality itself, and I went out after this wonderful package and they showed me these very papers. I had just a couple of days there, saw them, got photocopies of one, made a few notes on several others, and deposited this little bit in the archives in the Cavendish. Then I moved the English heaven and earth to try to get those papers transferred to Cambridge University, but as Mr. Strutt has already said, English heaven and earth is very difficult to move. I didn't have much success for various reasons, and a few months later I was immersed in

medieval astronomy and a few years after that I participated in the brain drain so I didn't succeed.

But I would like to say just two things. One concerns the Rayleigh material that is not here. I consider the apparatus, the instruments themselves, to be just as valuable and worthy as historic documents, and this extraordinary laboratory is preserved at Terling, the laboratory of the third and fourth Lords, in its original state with a notebook open on the table and a pen almost poised to write the next paper—with the original sealing wax and string. This I consider to be not a mere national monument, but an international one. People are fast disappearing that remember when physics was like this, and this is probably the last laboratory that preseserves intact this whole feeling for what apparatus was like. I desire very much and urge everybody that can to do something about it to make sure that the laboratory is kept there and studied, and if you do go and call on Mr. Strutt, I urge you to go to the converted barns and see this wonderful place.

The other point I would like to make is that the role of the third Lord, as Cavendish Professor, quite apart from the substantive content of his scietific work, as John said, his richest period, was a peculiarly crucial time in the whole development of physics as a profession. Maxwell had died young in his 40's in 1879 when Rayleigh decided eventually to make somewhat of a sacrifice and come to Cambridge. It was, I think, something of a struggle for him to take the job, but there simply wasn't anybody else. Kelvin wouldn't come, Maxwell had died—there wasn't another physicist, really. J. J. Thomson wasn't ready yet—he wasn't even ready until 12 years after he had the chair anyway—and in that period of 5 years that Rayleigh agreed and served out a bit, the Cavendish Laboratory pioneered the whole business, I think, of instruction at the elementary and advanced level in physics.

Physics research, especially experimental physics, I think, was born in the Cavendish Laboratory in this period. There is one marvelous letter in this collection from Arthur Schuster—I suspect a little more of that correspondence exists—in which they decide just what one is to do, what business is physics going to have in a university context. This was very non-obvious at the time. Rayleigh, of course, decided that the thing that you could do in a university that you couldn't perfectly well do on your own country estate, with your own apparatus, the thing you could do was to have the slave labor of graduate students. There and then the National Physical Laboratory was born as a device for doing the economically, then very important business, of standardizing electrical measurements and the important intellectual job of knowing what was meant by units in the first place.

I think he did a magnificent job with this from the scientific point of view, but not less important is his organizational challenge of knowing what to do with undergraduates and graduate students and colleagues in a university context of teaching

physics. The problem had been solved through chemistry, but it was not at all obvious that it would apply to the rest of the sciences. That decision was made there in that vital 5 years—the whole stage was laid—and J. J. Thomson and Rutherford afterwards made it possible. I think that this is a very important piece of social history that was achieved at that time.

DR. HOWARD: It is also true that Rayleigh had in the Cavendish two laboratory assistants, demonstration assistants, to help set up the experiments. One was a young man named Richard Glazebrook. The other was this young man who had just come from working in Berlin with Helmholtz, and his name was William Napier Shaw. As they helped the students assemble their experiment for the day, Rayleigh asked them to write down the instructions and they did this. I don't think they had mimeograph machines in those days, but they composited this into a little book by Glazebrook and Shaw called "Practical Physics," which was the first laboratory manual for physics. Glazebrook later edited the fine "Dictionary of Applied Physics."

Rayleigh also was chairman of the committee that recommended the formation of the National Physical Laboratory. He had the unusual good fortune of having a brother-in-law, Arthur Balfour, who was Prime Minister. Arthur Balfour was a bachelor, and when London got too hot he would go to Terling for the weekend. Rayleigh worked on him and said "You know, Germany has its PTB and the Americans are starting a National Bureau of Standards. We ought to have something like that." So Balfour said, "All right, we will form a committee to look into this and you are chairman of the committee." Rayleigh recommended Glazebrook as the first director and later did the same for the Meteorological Office with Napier Shaw as the first director.

This is another aspect of Rayleigh that I put on the agenda as the "professional activities," you might say, of Rayleigh, his influences that were not strictly scientific. There is one other thing I did want to say. A couple of people have brought up the point that there were little things that Rayleigh would mention and then not pursue further. And I think that now that these works are available, we ought to sort of systematically go through them and work on some of them because, for example, at the end of his argon paper, Number 214, concerning this new constituent of the atmosphere, he goes through a little list of why they named it argon, which means "without work." They just couldn't make it combine with anything.

They tried sulfur, strong acids, chlorine, hydrogen, attempts to induce chemical combination: they just couldn't make it react with anything. "We do not claim to have exhausted the possible reagents, but this much is certain, that

the gas deserves the name argon for it is a most astonishingly indifferent body inasmuch as it is unattacked by elements of very opposite character ranging from sodium and magnesium on the one hand to oxygen, chlorine, and sulphur on the other. It will be interesting to see if fluorine also is without action, but for the present that experiment must be postponed on account of difficulties with manipulation."

You know it was just 5 years ago some scientists made xenon and argon hexafluoride for which they probably will be Nobel laureates in chemistry one of these day. And it was all written right there in 1895; all they had to do was go ahead and do it. But you will find quite a few little hints. I hate to tell you this now that these books are so generally available.

Another thing relative to this laboratory at Terling. When Derek Price first heard I was going over, he said, "See what you can do about photo-documenting this place." I made a trip to the Nuffield Foundation for the History of Science at Regents Square to see Dr. Toulmin and their photogroup and he said: "Oh, unfortunately, our funds for the photo project are completely exhausted. We have nothing and this project went dormant for a year or so." And then later, in sending some of these reprints around to people who would be interested in Rayleigh, I sent a copy to Sir Lawrence Bragg who showed quite a bit of interest, and he immediately got in touch with the British Broadcasting Corporation. They have now put their photo project into this, and their plans are to come out and photo-document the place and prepare a film for a Sunday evening program called "Horizons" on BBC, in which they have looked at some of the origins of creative thought and things of that sort, and they want to do a program on Rayleigh. So, something is happening: that part of the documentation of the laboratory anyway. So it is not a dormant problem.

Well, one other thing that Rayleigh did while he was Cavendish professor that many of you are not aware of—he introduced the practice of tea in the afternoon—weekly tea. He wanted his students to mix it up a little bit, I guess to discuss their problems with each other; so I think we should stop for a tea break.

# COMMENTS BY PROFESSOR CHAPMAN ON THE INFLUENCE OF THE FOURTH BARON RAYLEIGH ON AIR GLOW AND AURORAL RESEARCH

DR. HOWARD: Well, I think we have had a very good discussion of John William, and now I would like to turn some atttetion to Robert, the fourth Rayleigh, and for the formal turning to this I would like some remarks from Professor Chapman. Professor Chapman has several interactions with Lord Rayleigh. Last year, for example, he was the most recent recipient of the Smithsonian Hodgkins Medal for distinguished atmospheric research and the first recipient of this medal was Lord Rayleigh, the third Lord Rayleigh, I believe in 1894. Professor Chapman is also a Smith's prizeman as was Lord Rayleigh. He has a list of honors equally long.

Now to his affiliation: even though he is an Englishman, he likes to spend the winters in Alaska and the summers in Nigeria, I believe just to make life more interesting, and it was an interesting way he came to our meeting today. He left Alaska, went to Seattle, and then to Minneapolis for some lectures, and then to Berkeley for some lectures, and then to Miami for some lectures, and last night flew up here to Boston and in a few days he is going on to England. But this is one way to stay young. I might mention that he likes to swim. When General Pinson heard that he liked to swim, he challenged him to a swimming match and Professor Chapman said, "Very well, but it has to be at least a mile because you could beat me in a shorter one." With those remarks, I would like to introduce Professor Chapman for some remarks on airglow, the aurora, and the fourth Lord Rayleigh.

PROFESSOR SYDNEY CHAPMAN: Yes, it was my honor and privilege to know the fourth Lord Rayleigh. I suppose that I must have seen the third Lord Rayleigh, because I must have become aware of him during my 7 years of undergraduate studies. Sounds rather bad being an undergraduate for 7 years, but I certainly must have become aware of his fame because the first 4 of these years were passed at the University of Manchester where Lamb, Reynolds, and Schuster were, all of whom were known to him, and, of course, truly estimated his greatness.

I am sure, almost sure, I must have seen the third Lord Rayleigh and not merely gotten a visual impression of him from this well-known picture. It must have probably been from the British Association which he attended rather regularly. I first went to that in 1913. I had much more of an association with Robert Strutt, but how and when it began, again, I cannot recall. If I have any diaristic or any other private written material about it, it is not accessible to me. Most of my papers of those many decades of my life are lying in a dark upper story of an old mill on the River Cam near Cambridge, and I haven't seen them for some years. I may look them up when I go to England in the next two weeks.

At sometime before I ceased to be an undergraduate at Cambridge, I was appointed Chief Assistant at Greenwich Observatory, along with Eddington. There were two chief assistants, the other one was Eddington. And the Astronomer Royal at that time was Sir Frank Dysen. He was very kind in taking us to meetings of the Royal Society and in other ways introducing us to London scientific life. I think it is likely that I first met and spoke with Robert Strutt before he succeeded to his father's title, that is, when he was Professor at the Imperial College of Science and Technology, London.

I spent the years 1914 to 1916 at Trinity College, Cambridge, as a Fellow and Lecturer, and while there I discovered mathematically the phonomenon of thermal diffusion in gases. I got a Cambridge chemist to cooperate and confirm this. That is to say, if you have a closed vessel, kept the two ends at different temperatures, with a mixture of gases in it, the composition of the hot and the cold ends would be different. Well, this chemist dropped it once, to demonstrate it, and I thought it ought to receive more attention. Actually, it was one of the ways later in which uranium was separated by thermal diffusion in making the first atomic bombs. So I went to the Imperial College with which I had had no connection, probably in 1917, to Callendar, then Head of the Physics Department who was an authority on steam, to urge that some experimental studies be undertaken on it. Well, he was polite, but not very encouraging, and I think he passed me on to his colleague, Professor Strutt.

Strutt had a life-long interest in gases like myself, and we had many other common grounds also, though this only gradually became clear. But of course, we are, in many ways, very unlike in antecedents and personality. One common

feature was that we both were or had been members, fellows of Trinity College, Cambridge. I think I didn't cut any more ice on that occasion with him either. I don't think he ever made any experiments on thermal diffusion or was ever interested in it. In 1919, when his father died and he succeeded to the title, he went to live at Terling and resigned his professorship at the Imperial College. And in that year also I left London and Cambridge to become Professor at Manchester, and I was also elected a Fellow of the Royal Society that year, so then I had some opportunity of coming into contact with him there. And 5 years later in 1924, I became head of the large Department of Mathematics at Imperial College and there began living for 22 years in London and took a part in London scientific life

Now by that time, Robert Strutt had published nearly 150 scientific papers of a most varied kind, and he continued to take a very lively interest in the Imperial College, becoming a member of its governing board, which is equivalent to the Regents of an American university, and in his late last years he served as its chairman: his last 11 years. I suppose we met occasionally at the Royal Society and though both of us served at different times on the Council of that Society—I for 3 years, he for more—I don't think they coincided. He expected mathematics, but his bent was not mathematical. But we had one subject of common interest, namely the aurora, the aurora borealis, and he wrote several papers on that before this period.

There was a very active sunspot period in 1921 and a very great magnetic storm when the aurora was seen at Samoa. (The next time the aurora was seen at Samoa it was seen in a very unexpected direction—to the north instead of the south—and it was produced when a nuclear bomb exploded over Johnston Island in the Pacific.) My attention was drawn to the aurora because of my interest in geomagnetism stimulated by Dyson and Schuster. There was a magnetic observatory at Greenwich Observatory which had become out of date, and one of my first jobs was to renew the observatory. So I had to learn something about geomagnetism, of which I knew nothing at the time, and this is closely linked with the aurora.

One of his father's many great achievements, of course, was the explanation of the blue of the sky, but Robert's published studies of atmospheric optics began only in the middle period of his career, around 1910, when he was 41 years old. It was his colleague, Alfred Fowler, another professor of physics at the Imperial College, later my colleague and good friend, who drew Robert's attention to some bands in the spectrum of Sirius taken by the astronomer, Arthur Sampson. Perhaps I might quote a little from the biographical notice of Robert John Strutt on this point. It is well, I think, to hear some of Robert John Strutt's own words at this celebration.

It says: "One day in 1916, with a query, my colleague, Alfred Fowler, asked me if I was very busy. I said not more than usual. He then showed me some photographs of the spectrum of Sirius taken by Sampson of Greenwich Observatory." He then goes on: "But it seemed likely that they were due to absorption in the earth's atmosphere, but no suggestion had been made as to the constituent responsible. Fowler had investigated the spectrum of the setting sun and had found these bands there also. Though owing to the abundance of absorption lines in the sun spectrum, they were not so clear as in Sirius. It had already been suggested by Hartley of Dublin that the fading out of the spectra of the sun and stars in the ultraviolet might be due to atmospheric ozone, which in laboratory tests cuts off all ultraviolet light from about wavelengths 3000 and under. This was the position when Fowler first discussed the matter with me. He proposed to investigate the arc ozone bands anew and see if they would fit the bands which he had found in the solar spectrum. He said that he had no leisure to do it and I agreed to help as he wished"(and then he describes how he did it in the laboratory), "and as soon as the plate was developed, I strongly suspected seeing the same bands that Fowler had found in the solar spectrum. I took it to him and wiping the back of the plate dry with a glass cloth, we compared the spectra as well as we could subject to the limitation that the wet films could not be placed in contact. We saw at once that the bands in the solar spectrum fitted those in the ozone absorption. It was a dramatic moment and this was the first absolutely definite proof of the presence of ozone in the atmosphere, though it had been long suspected."

I might mention here that this research, you see, was stimulated by Fowler and Robert Strutt was, on the whole, a solitary worker. As far as I know, he didn't have any pupils who became research men, he didn't found a school, and he wrote very few joint papers and only with three men did he ever write a joint paper. He wrote two with Professor Alfred Fowler, his colleague at Imperial College, and five or six with his chemical colleague, H. B. Baker, and the only other man was the Astronomer Royal, Spencer Jones, with whom he published a paper on the night sky. Then Robert Strutt took up the study of this ozone absorption on his own lines.

You remember this was in 1916 that this work was done, and it was published in 1917 by Fowler and Strutt. But Robert Strutt began making experiments on his own. He wanted to see the effect of the absorption of ozone when a beam of light, which it absorbs, was passed between two places in his neighborhood, between which there was as much as there is in the whole atmosphere. He says, "At that time a blackout was prevailing directed against the Zeppelins, a rigid airship, for airplanes were not yet regarded as a serious menace. It was necessary to get permission to show the light across the valley of the Chelmer and after some preliminary trouble in trying to make the local military understand why I wanted

it (they couldn't understand the idea of a research not directed to military ends), I got the required permission through my uncle, A. J. Balfour, who was then a member of the Government." And he got this beam of light reflected through this amount of air and found that the absorption was much less than had been found in the sun spectrum, and in this way he proved that the ozone was not distributed uniformly through the atmosphere so that its density increased downwards, but that it must be situated higher in the atmosphere. I think this paper was published in 1918 or 1919.

From the time of that work on ozone, his interest in atmospheric optics was frequently shown by many interesting and by some very important papers which continued until 1935. I think he didn't write anything on the atmospheric optics after that date. Around about 1921, as I said before, I went to the Imperial College. He had been studying the auroral spectrum and he was very surprised to find that the prominent line in the spectrum of the aurora, the green line, which in the early days had been ascribed to krypton, was less intense at his northern home, Beaufront Castle, his wife's northern home near Newcastle, than at Terling in southeast England. This seemed to him very surprising, and he continued to make observations on the intensity of this green line and found that it didn't seem to be associated with magnetic disturbance. So later he was able to make a distinction between the aurora, ordinarily so-called, which becomes more frequent as you go up towards higher latitudes until you get to the auroral zone, beyond which it becomes less frequent again. A distinction between that aurora associated with magnetic disturbance and this auroral line in the night sky, which he called the non-polar aurora, and which in recent years on the suggestion of Dr. Elvey, later director of the Geophysical Institute of the University of Alaska, is now called the airglow, or the night glow or the dayglow, according to the time in which it is observed.

He obtained the spectrum of the aurora and found there were nitrogen bands in it at times and other times naught. He devotes a little attention to the fact that this line did not agree in wavelengths with the krypton line, and I must say I am a little surprised that he took this suggestion so seriously because krypton is a very heavy gas, and in a paper Robert Strutt published about 1925 he showed that he believed that the atmosphere above 130 km, which is the kind of auroral level, consisted mostly of helium and he was very surprised that this aurora did not show the spectrum of helium. Well, he made an important step in recognizing that there are two kinds of light from the sky. One which is associated with magnetic disturbance and the aurora seen mostly in higher latitudes and the airglow which is a phenomenon observable all over the earth and which is more in lower latitudes than in higher.

I used to lecture sometimes on the aurora in those years in the 20's, and I shudder to think now about some of the things I must have said about the aurora in those days because it was very speculative and I remember Robert Strutt speaking a little scathingly about what passes for knowledge on various subjects in reference to speculative ideas. Alfred Fowler once told me that he would knock himself on the head if he couldn't find the source of this green aurora line, but fortunately, he didn't do it until the source had been discovered by the Canadian Physicists, McLennan and Shium, Toronto, in 1924. They found that it was due to a transition between two metastable states of atomic oxygen.

I don't know what turned myattention to the problem of atmospheric ozone, but around about 1928 or 1929, I began to think how it could exist in the upper atmosphere. Its height was then thought to be about 50 kms. Now its maximum is more like 30 or 35. I worked out a photochemical theory of the subject, though beginning with practically no knowledge of the subject, and I found my chemical colleagues very unfamiliar with the kind of chemical actions which would go on in a great volume of air with no boundaries. And from the existence of the ozone layer, 30 or 40 km, I inferred that in the upper atmosphere the oxygen must be largely disassociated, so that oxygen not being molecular would be atomic.

And then I tried to find out what could be the source of this oxygen line which appeared not only in the aurora, which was believed to be excited by fast-moving particles descending through the atmosphere, but on quiet nights, every night. I proposed a reaction and a few years ago a Stanford physicist named Young came to the Geophysical Institute to give a lecture on this subject. He indicated that he disproved this explanation of this line by me, but in the last issue of Scientific American he has turned around and later research has proved this explanation—the reaction, a triple collision between oxygen and atoms leading to recombination of two of them to form a molecule with the excess energy going into the third atom which enables it to emit this light—is valid.

Well, to me it seemed one might apply numbers to this problem, and knowing that Lord Rayleigh had spent a great deal of effort in long observing this green light in the night sky, I pointed out to him the interest and importance of trying to get a quantitative measure of the intensity of the light which would enable us to know how many of these atomic transitions, the excited oxygen atom, were taking place in a $cm^2$ column from the ground up to the top of the atmosphere.

Though he hadn't thought of this way of looking at it before, he realized its importance and he went to work and I understand it was an extremely difficult research to do. He finally succeeded and published a paper on this subject in 1930, obtaining the absolute intensity of the light. It is this paper, this determination by him of the intensity of a sheet of luminosity uniform over the heavens, that lead two or three physicists, greatly interested in the airglow some few years

ago, to propose that a unit of this intensity should be named after Rayleigh and so it is today. So Robert Strutt's name is perpetuated by having an important physical unit named after him. I think it may well be that there will be many young men in the future who, perhaps not interested in the mathematics and the sound or optics of Robert John Strutt's great father, will know the Rayleigh name through this unit and who may, well, if they hear that Rayleigh had done something on sound, telescope the two personalities and think there was one Lord Rayleigh whose interests were so wide as that.

It certainly is the case that Robert Strutt's interests were very widespread. They were not mainly mathematical. I was looking through his papers the other day in Miami University Library. I see that in one written in 1940 he referred to having referred theoretical discussion to me. I had quite forgotten that.

I think his most important scientific work was not in atmospheric optics, but in his work on radioactivity and the radioactive content of minerals and the helium content. For it was by this that he overthrew the estimates of Lord Kelvin, who was a great friend of the family, that the earth could only be 20 to 100 million years of age. The biologists and geologists had tried to squeeze their time scale into this 20 to 100 million years because of Kelvin's great authority, and when Robert Strutt proved that you could expand it very very greatly into billions of years, one geologist said: "It's very hard to get the chill of poverty out of one's bones." He found it very difficult to stretch his time scale in the direction from which he had compressed it.

I have many pleasant memories of Robert Strutt's hospitality to me and my wife in his London home. On the occasion of the Royal Society's anniversary, he would give a dinner party to which also foreigners, Americans, and other visiting scientists would be invited. I remember on one occasion he said that "If you saw an old lady crossing the streets in entire disregard of London traffic, you would know it was his mother, the sister of Arthur Balfour." He had a very pleasant humor and on an anniversary in 1942, which was also the Tercentenary of Newton's birth, and though it was war time, a celebration of it was held and one feature of it was lectures on the work of Newton. And Robert Strutt gave a lecture on Newton's experimental work and he repeated Newton's experiments to prove that the refrangibility of light was a function of color and to the same color always was the same refraction. The Royal Society almost always held its anniversary meeting on November the 30th, St. Andrews Day, and on November 30 the afternoon atmosphere in London is not very good; this is one of his remarks from that lecture: "In spite of our increased control over nature, we cannot produce a beam of sunlight at pleasure late on a November afternoon, so I must be content to use the electric arc in this experiment."

I was kindly invited, my wife and I, to spend a weekend at Terling, some year before the second World War; I forget which it was. Unfortunately, that weekend, Lord Rayleigh was in bed with influenza and a thick fog descended over Essex so that one could only see a few feet in front of one. It was a rather difficult occasion. Sir Charles Parsons and Sir Richard Gregory, the Editor of Nature, were among the guests, and they went to visit him in his bedroom. I was not invited to do that. No, I think I missed him that way while seeing the laboratory which has been spoken of. Though it hasn't to do with science, perhaps I might mention the last phase of our relationship which was not quite so pleasant.

As I mentioned, Lord Rayleigh was Chairman of the Board of Visitors of the Imperial College and the latter part of my time there, the head of the College, he was called Rector which corresponds to the President of an American university, was Sir Henry Tizard, who you may remember was involved in famous difficulties with Lord Charwell (Lindeman, Churchill's scientific advisor). Tizard resigned late in the war, or perhaps it was just a little after, to become president of Magdalen College, Oxford, and the question of a new Rector came up. Now, I don't know whether there are faculty representatives on the Boards of Regents of America, what you may call State Universities, but at the Imperial College, there were four faculty representatives on the Board of Governors and by that time I was in my 50's and I had become senior enough to become one of these four and we talked it over among ourselves and came to the conclusion that it would be a good thing if on the small sub-committee we knew the Board of Governors would appoint to select a new Rector, the staff should be represented by one of us. I was deputed by my colleagues to make this proposal and I have never seen anything so electrifying. It really reminded me of Oliver Twist's demand for more oatmeal cereal—more porridge. The whole Board was absolutely silent at the idea that the staff should be represented on this very confidential committee. And I thought Lord Rayleigh, having been a professor himself, would have some sympathy with the idea, but it had never been done before and he was of somewhat a conservative frame of temperament and so it was declined. But nevertheless the door was kept a little ajar because Lord Rayleigh was to telephone me from time to time to tell me what the committee would deliver, who they were thinking of finding, and inviting my comments on it. There was no unfriendliness about it, but there was certainly a little rift.

I have said how astonished the Board of Governors were at the request that a staff member should be put on the small Committee to choose a new Rector of the College, but next time there was a vacancy the proposal was resubmitted and the idea got around that it was quite a useful thing to do and it was done with no trouble whatsoever.

But my memories of him are a delightful lecturer, with much humor, a very versatile scientist who had very very great scientific accomplishments to his credit, especially in the fields I mentioned, but also in many others, and I am delighted that his papers are now in such good custody. I am delighted to hear that his works are to be collected. They certainly should be and I understand there is to be a film about father and son made by the BBC. I hope that also will come to pass.

Thank you.

# DESCRIPTION OF THE MATERIAL PERTAINING TO THE FOURTH BARON RAYLEIGH

DR HOWARD: Thank you very much Professor Chapman. We inverted the order a little bit to get the maximum audience alertness right after coffee for Professor Chapman's remarks. But I should mention what we have of Robert's in our collection. We do have his 22 notebooks, and I suppose you noted that we put the originals in the glass cases over by the door there, each in individually fitted boxes. Over here, on the side, we have bound a set of Xeroxes of the notebooks. These are available in the Library for people to use. The blue set is John William, the third Rayleigh, and the red set below are the 22 notebooks of Robert, the fourth Rayleigh.

When Dr. Price said that you could go into that laboratory and see one notebook there with the pen held just about ready to write, I am afraid that isn't quite true because here it is—but it will go back there. This is the last notebook, the one that was lying in the laboratory, and this is his rough notebook in which he wrote his day-to-day data before he transcribed it into his more permanent notebook. So these we are going to microfilm and then return, so the pen can again be put on the notebook in the laboratory.

But in addition, Victor Twersky mentioned the biography that Robert wrote of his father (there is a copy of it back there). I have borrowed from the family, from Charles Strutt, Robert's personal copy of his life of his father, which was an interleaved copy, a blank page between every two printed pages, where he had written little marginal changes, particularly remarks that he didn't want to put in print because certain people were still alive at the time he published that book

in 1924. Particularly, the personality problems between Ramsay and Dewar at the time of the discovery of argon. He mentions some of the discussions of who were the alternative candidates to be Cavendish professor when Rayleigh stepped down; J. J. Thompson ended up with this. But there were other people being considered such as Glazebrook, for example, and Robert didn't want to discuss that sort of confidential matter while all these people were very much alive. But I borrowed that volume from the family and here is a Xerox with the interleaved comments and this we are considering publishing and updating because Rayleigh on Rayleigh has been out of print for quite a few years.

I am a little surprised that it is so hard to get because it was one of those London book-of-the-month club selections at the time it came out in 1924. One might have thought there would be a lot of copies, but it is a very difficult book to find. But in addition to that, I have quite a few individual papers of Robert Strutt, galley proofs of some of his early radioactivity articles. This helium clock that he devised, working jointly with Rutherford, was indeed one of his great accomplishments. He took a mineral, uranium or radium, determined the amount of helium in this, put it in a sealed capsule for, I believe, roughly a year, and then re-determined the helium. He was then able to say that this mineral has to be at least 20 million years old. And this frustrated Kelvin because the only way Kelvin could explain why the sun shines was by a gravitational collapse of the sun and it just couldn't be that old or it would have been an impossible dimension to be giving out that kind of energy.

We also have some of Robert's books from that period. He wrote a book on Becquerel rays, and over here in a set of 12 volumes that was put together in brown folders, is a complete set of his 301 papers—the number that Sir Alfred Egerton listed in the obituary notice.

I have assembled Xeroxes from the published literature and collated them into those books; I might add that Sir Alfred must have been under some duress to write this particular memoir, because he missed quite a few extra papers. I have gone through a little more carefully and I am nearly up to 350 instead of 301. In fact, he missed some that were fairly substantial papers. He missed several little letters, but one or two were big papers. But this is the set of papers that we feel could be published, possibly in a matching kind of set to the collected papers of the third Rayleigh. They run about the same length per paper, so 2 volumes would perhaps accomplish the 300 or so papers of Robert. Well, that is a brief discussion of our archives of the fourth Rayleigh.

Well now, is Dr. Oldenberg still here? I think he may have gone off. I had wanted him to make some remarks. He is still actively pursuing active nitrogen, which was Robert's other major interest. Robert's contributions, the early ones, were his studies of radioactivity, and really the concept of a helium clock that he discovered then is very similar to the carbon clock that Libby worked on and won

a prize for. Except, of course Libby had the very clever additional feature that this would tag living material from inanimate material. And then his work on gaseous luminous discharge phenomena was his other major interest. I don't think we have anybody here who is properly prepared to speak on this. Professor Roach at Boulder had wanted to come but got lured off to an Astronautical Meeting at Williamsburg so he is not here. But it was Professor Roach, who is now at Boulder, Colorado, along with Joseph Chamberlain and Don Hunten who co-sponsored a proposal that we name the unit of airglow the "rayleigh." The "rayleigh" is defined as a surface brightness of $10^6$ photons per $cm^2$ column per second or per hemisphere if you don't want to say column. Isn't that right? OK. But that is a very awkward thing to have to say over and over everytime you are specifying the airglow, and I think they were very quick to adopt this unit as a rayleigh, which puts airglows in the range one to hundreds of rayleighs, and auroras from a few hundreds to 8000. It is a very convenient unit for airglow measurements.

I once heard Frank Johnson, who is a very active airglow man, describe how one can look at the airglow if you are ever curious to do this. Go out on a dark night when there is no moon and let your eyes become dark-adapted, and then hold your hand up like this and look at the sky; if your eyes are properly dark-adapted, you will be able to see your hand silhouetted against the sky. The airglow is very faint: about a 20th of the brightness of a full moon, but it is a very difficult thing to see normally unless you are looking at a brilliant aurora display, or something of that sort.

The fourth Rayleigh also did quite a bit of work on the Green Flash at sunset on which, I believe, the great authority is now Father Jonkes at the Vatican Observatory. And Rayleigh also re-examined many areas that had interested his father, such as the metallic sheen of insects—why do Japanese beetles look so metallic ? Michelson had also worked on this problem. He wrote papers on the strength of materials; the bending of marble and things of that sort. He tried also to keep the same simple approach to experimentation that his father had had. He had only one assistant. He preferred to work alone. He worked mostly at Terling. He had the same thriftiness—one might almost say frugality—of not wanting fancy equipment. He used simple equipment and home-made equipment wherever possible, and he venerated his father's laboratory so that he tried not to change anything from the way it had been. Even when electricity was brought in, it was sort of smuggled in so that it didn't change the appearance of the laboratory. But, well, are there any remarks? Who have I missed? We are running a town meeting here—a Quaker meeting if you want to put it that way.

## CONCLUDING REMARKS

DR. F. DOW SMITH: I have a couple of very brief things John.

DR. HOWARD: All right. Dr. Smith, the hero who gave us all this beautiful reproduction of Paper 5.

DR. F. DOW SMITH: I received a letter from Dr. Duncan Macdonald of ITEK which is addressed to John which I will leave with him, but I thought I might read a portion of it.

Dear John: (He comments on a standing commitment that prevented him from being here, but I think the rest of it reflects a number of our thoughts. I thought I would just read it.)

"While the United States Air Force joins with the scientific communities of education and business to provide appropriate recognition of this occasion, the warmth, the depth, and the strength of their participation is also a tribute to you, John, of your own dedication and devotion over the years which has made this occasion possible. My congratulations to you and your staff. My regards to my many participating friends and with regrets that I shall not be there."

But I think today represents the result of a very strong personal effort by you and many others. One other very brief thing that I would like to say: When this material was first obtained, I remember John told us locally about it and we were

particularly entranced. I was particularly interested in the notebooks, which John referred to as Lord Rayleigh's diaries. These were the books in which each day, I understand, Lord Rayleigh wrote down the results of that day's experimentation, and were of course of very great interest to us. I think you have all seen the pictures of Terling and I am looking forward to exercising the invitation that has been offered today to visit that most beautiful estate. I think some of you have also seen some pictures of Lord Rayleigh's dairies, which now makes productive use of much of what must be beautiful agricultural country. Now, I am not enough of an agricultural man—in fact, I am not an agricultural expert at all—so I am not really qualified to comment on the worthiness of the impressive awards that I see have been given to the productivity and some of the fine animals that have been bred on the farm. But one thing that has impressed me today is that this long tenuous series of events that led from the curiosity of a scientist at AFCRL through the contributions of librarians and other individuals; the whole sequence of events that led to the archives being here. It all seems so very chancy and it occurred to me that in looking at this pamphlet on Lord Rayleigh's dairies, that when the final check was written, wasn't it fortunate the purchasing agent at Air Force Cambridge Research Laboratories in writing for Lord Rayleigh's diaries, knew how to spell.

DR. HOWARD: Well, fortunately, not very far away from them is a U.S. Air Force base which consumes a sizeable part of their milk. Isn't that right ? They drink lots of milk in the Air Force because Air Force men, in general, are very young. Well, let me see—several people didn't speak up and I won't call on them. For example, there are many fields we didn't touch on. Frank Archibald is a lubrication engineer and likes to point out that Rayleigh wrote the first fundamental papers on the theory of lubrication. He is one of the big pillars in that field along with Osborne Reynolds and people of that sort. Who else? Thermodynamics—here is Erwin Hiebert, who has also written several detailed papers on the argon period.

DR. ERWIN HIEBERT: If you gave me an hour, John, I could tell you a great deal about how the work of Lord Rayleigh revolutionized molecular chemistry in terms of the kinetic theory of motion and so forth, but I am not going to.

DR. HOWARD: You can't have it anyway.

MR. FRANK ARCHIBALD: John, if you give me one minute.

DR. HOWARD: Very well.

MR. FRANK ARCHIBALD: I wasn't going to speak, but Professor Lindsay encouraged me a bit. He has expressed a sense of inferiority to the elegant physics of today. I am a long way from Professor Lindsay, I assure you, but I did make these preparatory things to say in case I should have been called upon or had an opportunity to read them. I am bound to get to say them.

The subjects that Rayleigh studied, investigated, and added to so much have been, in many cases, taken over by engineers—particularly in acoustics—and also notably in elasticity and hydrodynamics. I prepared this little table of physics references.

In Long's theory of elasticity, the references to Rayleigh are 41. The only greater number is to St. Venant, the famous French elastician with 47. In Lamb: Stokes is 57, Kelvin 55, and Rayleigh 102. Rayleigh dominates the whole field of theoretical hydrodynamics. In Timoshenko, the patron saint of American Engineering, Rayleigh is the top reference in "Vibrations," with 11, and much use is made in engineering of the Rayleigh-Ritz method, the inextensible bending of shells, the method of finding low mold vibrations of complicated systems, dissapation functions, and so on.

DR. HOWARD: Dr. Benedek pointed out that there was an early paper of Rayleigh which really anticipated Raman, in fact 30 years before Raman.

DR. GEORGE B. BENEDEK: There is a letter that Rayleigh wrote to Michelson in connection with Michelson's method of the visibility of fringes. This bears on the question of resolving the fine structure of spectral lines and speculating further beyond Michelson's method of fringe visibility. Rayleigh asks himself what could be the source of the broadening or of the splitting of spectral lines, and I think it is thrown out again in his letter as a suggestion, much like the other suggestions that we have heard today which later led to very great things. He suggests that the rotation and the vibration of the molecules also can affect the light that is scattered from the molecules, and writes a very simple equation showing that the light that is incident on a rotating molecule will be scattered with a frequency different from the incident frequency and the different frequency should appear both above and below the incident frequency line. This is very close, if not precisely the same as the Raman effect, which was found by Raman in 1928, and for which Raman won the Nobel prize just for the demonstration that the light coming out would have a different frequency than that coming in. I think this is another example of Rayleigh

just throwing something out which was later picked up by someone who demonstrated it and in this case won a Nobel prize.

DR. HOWARD: Well, I have one little final thing I thought I might mention. In 1900, which was a couple of years before the Wright brothers, Rayleigh at a Royal Institution lecture gave a lecture on flight and he discussed the soaring of birds and kites (which are not free-flight because there is a string attached and one can figure out a table of forces), but he wanted to consider whether or not artificial flight would be a practical thing. I am reading from the account of the lecture reported in Nature. "He thought it was more or less a question of the speed of the horizontal motion. A bird did not use a revolving mechanism like a screw propeller to propel itself, but he had no doubt that a revolving mechanism was the most suitable for artificial flying machines. Whether the difficulties of these would be surmounted, he did not know, but he was disposed to agree with Mr. Maxim (the inventor of the Maxim gun, the recoilless rifle) that it was mainly a question of time and much money. Still he did not think that flight would ever be a safe mode of conveyance to those who were desirous of going out for a day's shopping for it was hard to see how alighting on the ground could ever be rendered quite free from danger, but as Mr. Maxium once remarked—the first use of flying machines would probably be for military purposes and they had not yet succeeded in making war quite safe."

Well on that happy note, I think we probably should consider our Rayleigh Archives dedicated.

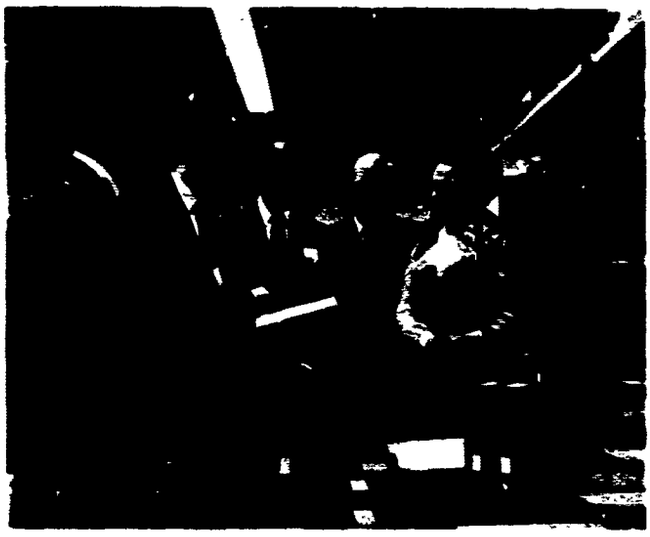
Thank you very much for attending.

# PICTORIAL REVIEW OF RAYLEIGH DAY CEREMONY

Brigadier General E. A. Pinson,
Commander OAR,
Welcomes Attendees at Rayleigh Day Ceremonies

Mrs. Charles R. Strutt, Professor Robert A. Smith,
Director, Center for Materials Science and Engineering, M.I.T.,
Dr. John N. Howard, and Brigadier General E. A. Pinson

Colonel R. F. Long, Commander, AFCRL

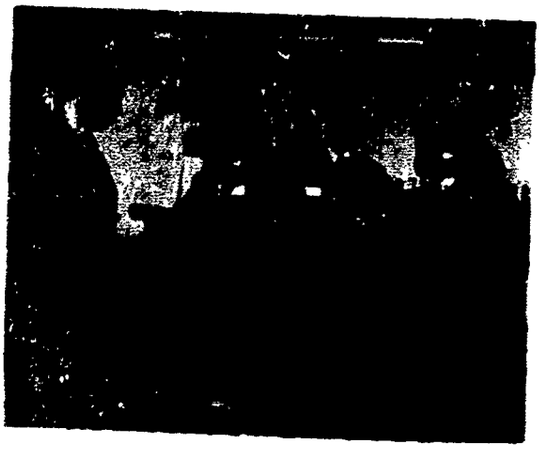


Brigadier General E. A. Pinson, Dr. J. N. Howard,
the Honorable Charles R. Strutt,
Colonel R. F. Long, and Mrs. Strutt

Colonel James L. Dick, Vice Commander, AFCRL, and the Honorable Charles R. Strutt

Dr. John N. Howard, Chief Scientist of AFCRL and Master of Ceremonies for the Dedication of the Rayleigh Archives, with the Honorable Charles R. Strutt, Son of the Fourth Lord Rayleigh, and Mrs. Charles R. Strutt

The Honorable Charles R. Strutt Describing Terling to Professor Robert S. Shankland, Department of Physics, Case Institute of Technology. It was at Terling, the Country Estate in Essex of Lord Rayleigh, that a Stable was Converted into a Private Laboratory by the Third Lord Rayleigh

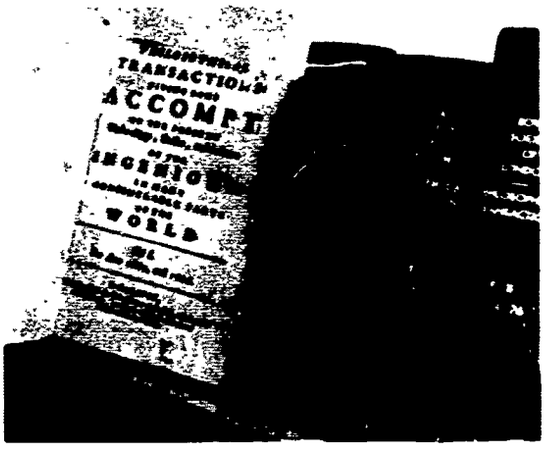


Dr. Charles Weiner, Director, Center for History and Philosophy of Physics, AIP; Dr. Jules Aarons, AFCRL; Colonel James L. Dick; and the Honorable Charles R. Strutt

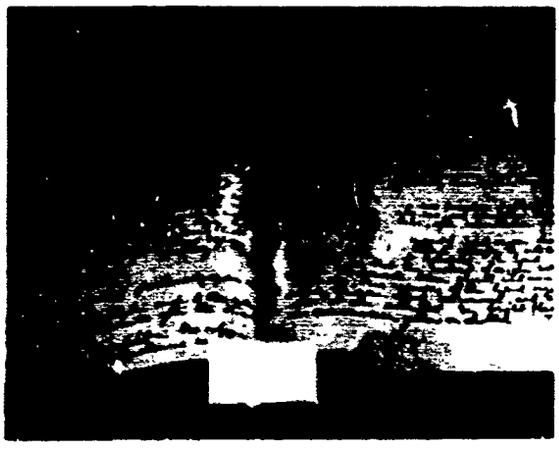
Professor Derek de Solla Price; Professor Sydney Chapman, University of Alaska and NCAR, Boulder, Colorado; Professor Willis E. Lamb, Physics Department, Yale University; and the Honorable Charles R. Strutt

Professor Derek de Solla Price, Avalon Professor of the History of Science, Yale University; Professor Gerald Holton, Department of Physics, Harvard University; and Dr. J. N. Howard

Dr. Thompson Webb, Jr., Director, University of Wisconsin Press; Dr. J. N. Howard; Professor Erwin N. Hiebet, Department of the History of Science, University of Wisconsin; and Mr. Frank R. Archibald of Comstock and Wescott, Cambridge, Massachusetts

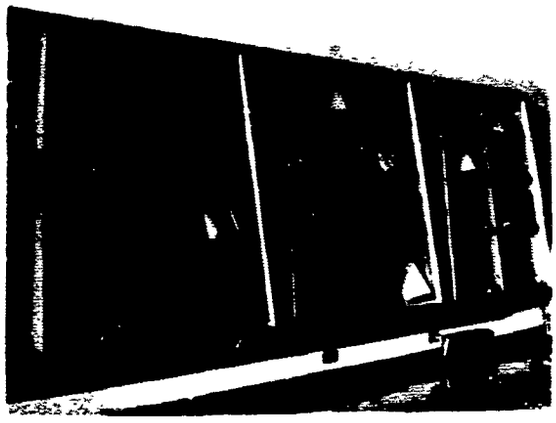



One of the Rare Volumes from AFCRL's Library on Display During Rayleigh Day Ceremonies. Shown is Volume I of Philosophica. Transactions of the Royal Society of London. It was printed in 1665



One of the Original Notebooks of the Fourth Lord Rayleigh in AFCRL's Collection. Reflected from the Glass Cover on the Case is the Face of the Honorable Charles R. Strutt

Dr. Jules Aarons Inspecting the Display of Rayleigh Material



Some of the Rayleigh Notebooks Displayed During Rayleigh Day Ceremonies at AFCRL

General View of Audience. Shown are Professor Chapman; Professor Gerald S. Hawkins, Director of the Observatory, Department of Physics-Astronomy, Boston University; Brigadier General E. A. Pinson; Professor Sanborn C. Brown, Associate Dean Graduate School, M.I.T.; Honorable Charles R. Strutt; Dr. Van Zandt Williams, Director, American Institute of Physics, Dr. Victor Twersky, Head of Research, Sylvania Electronics Systems; Professor R.A. Smith; and Colonel R.F. Long

General View of Audience: First Row: Professor R.S. Shankland, Dr. J.N. Howard, Honorable C.R. Strutt, Professor R.A. Smith. Second Row: Professor G.S. Hawkins, Dr. F.D. Smith, Professor S.C. Brown, Dr. V. Twersky, Colonel R.F. Long. Third Row: Dr. R.W. Fenn, Dr. R. Penndorf, Dr. V.Z. Williams. Fourth Row: Professor W.E. Lamb, Professor G. Holton, Professor E.N. Hiebert

General View of Audience: Dr. Gonzalo J. Hernandez, Professor Otto Oldenberg, Professor Willis E. Lamb, Professor Gerald Holten, Professor Erwin N. Hiebert, Dr. Charles Weiner, Dr. Thompson Webb, Jr., Dr. L.M. Hollingsworth, Dr. John S. Garing, Dr. George Vanasse, Mr. John H. Durston, Dr. Jules Aaron, and Dr. E.M. Dewan, Jr.

# Appendix A

## Attendees at Dedication Ceremony

Honorable Charles R. Strutt and Mrs. Strutt
Berwick Place
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Vice Commander
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Bolt, Beranek, and Newman, Inc.
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Dean Harvey Brooks
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Harvard University
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Professor Sanborn C. Brown
Associate Dean of the Graduate School
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Dr. B.F. Kingsbury
Educational Services, Inc.
Watertown, Massachusetts 02172

Dr. Winston E. Kock
Director
NASA-ERC
Cambridge, Massachusetts 02139

Professor Willis E. Lamb
Physics Department
Yale University
New Haven, Connecticut 06520

Professor R. B. Lindsay
Dean of the Graduate School
Brown University
Providence, Rhode Island 02912

Professor D. J. Lovell
Institute of Science and Technology
The University of Michigan
Ann Arbor, Michigan 48107

Professor Otto Oldenberg
Harvard University and AFCRL
Boston, Massachusetts

Dr. Rudolf Penndorf
AVCO-RAD Division
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ITEK
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Director
Center for Materials Science and Engineering
M. I. T.
Cambridge, Massachusetts 02139

Dr. Victor Twersky
Head of Research
Sylvania Electronic Systems
Mountain View, California 94042

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Bedford, Massachusetts 01730

Dr. Thompson Webb, Jr.
Director
The University of Wisconsin Press
Madison, Wisconsin 53701

Dr. Charles Weiner, Director
Center for History and Philosophy of Physics
American Institute of Physics
New York, New York 10017

Dr. F. Karl Willenbrock
Associate Dean
Division of Engineering and Applied Physics
Harvard University
Cambridge, Massachusetts 02138

Dr. Van Zandt Williams
Director
American Institute of Physics
New York, New York 10017

# Appendix B

## A Summary of the AFCRL Rayleigh Archives

### 1. THE EXPERIMENTAL NOTEBOOKS

This collection consists of 12 notebooks (for the time period 1862-1919) of the third Baron Rayleigh, one in the hand of Mrs. E. M. Sidgwick (for the time period 1880-1881), and 24 notebooks of the fourth Baron Rayleigh (for the time period 1910-1947). The contents of these notebooks are described in detail in an article by J. N. Howard, Applied Optics 3: 1129 (1964), except for the two last notebooks of the fourth Baron, which were only recently added to the collection: No. 23, "Experimental Notes," 26 October 1944-30 October 1947, and No. 24, "Rough Note Book," 6 October 1947-30 November 1947.

### 2. MANUSCRIPTS OF PUBLISHED PAPERS

The Rayleigh Collection contains the following 187 original manuscripts of the 446 published papers of Lord Rayleigh (J. W. S.). The numbers given are those assigned to the papers by Rayleigh in the collected "Scientific Papers" (Dover, 1964).

| | | | | |
|---|---|---|---|---|
| 8 | 213 | 230 | 251 | 268 |
| 45 | 214 | 231 | 252 | 270 |
| 74 | 216 | 233 | 257 | 272 |
| 108 | 217 | 238 | 260 | 273 |
| 151 | 223 | 239 | 261 | 274 |
| 199 | 224 | 242 | 262 | 276 |
| 203 | 225 | 243 | 263 | 277 |
| 208 | 227 | 246 | 264 | 279 |
| 211 | 228 | 247 | 265 | 280 |
| 212 | 229 | 249 | 267 | 281 |

| | | | | |
|---|---|---|---|---|
| 282 | 298 | 311 | 321 | 331 |
| 283 | 299 | 312 | 322 | 332 |
| 284 | 301 | 313 | 323 | 333 |
| 285 | 302 | 314 | 324 | 334 |
| 288 | 303 | 315 | 325 | 335 |
| 289 | 304 | 316 | 326 | 336 |
| 290 | 306 | 317 | 327 | 337 |
| 291 | 308 | 318 | 328 | 338 |
| 292 | 309 | 319 | 329 | 339 |
| 296 | 310 | 320 | 330 | 340 |

| | | | | |
|---|---|---|---|---|
| 342 | 352 | 366 | 377 | 387 |
| 343 | 354 | 367 | 378 | 388 |
| 344 | 355 | 368 | 379 | 390 |
| 345 | 357 | 369 | 380 | 391 |
| 346 | 358 | 371 | 381 | 392 |
| 347 | 359 | 372 | 382 | 393 |
| 348 | 360 | 373 | 383 | 394 |
| 349 | 362 | 374 | 384 | 395 |
| 350 | 364 | 375 | 385 | 396 |
| 351 | 365 | 376 | 386 | 397 |

| | | | | |
|---|---|---|---|---|
| 398 | 408 | 419 | 428 | 438 |
| 399 | 409 | 421 | 429 | 442 |
| 401 | 411 | 422 | 430 | 443 |
| 402 | 412 | 423 | 432 | 444 |
| 403 | 413 | 424 | 433 | 445 |
| 404 | 415 | 426 | 434 | 446 |
| 407 | 418 | 427 | 436 | 3 unnumbered |

Some rough calculations are written on the back of pages of earlier manuscripts; we may ultimately re-assemble a few more manuscripts from this source. Of 187 manuscripts in our collection, only 6 are from the first 200 papers Rayleigh wrote (3 percent), as compared with 181 of the remaining 246 (73.5 percent). Although Paper 45 was not published until 1877, it actually antedates all of the other numbered papers, having been written for the B.A. meeting of 1868. Rayleigh completed Paper 444 five days before he died. Papers 445 and 446 were published after Rayleigh's death in 1919, but both of them probably date from 1917. Three other papers were not included in the collected papers.

## 3. THE ROUGH NOTES AND CALCULATIONS OF LORD RAYLEIGH (J. W. S.)

The following material has been thus far indentified. (The number is that of the published paper to which the material relates.)

| | | | | |
|---|---|---|---|---|
| 7 | 75 | 149 | 164 | 211 |
| 10 | 77 | 150 | 178 | 212 |
| 44 | 78 | 151 | 180 | 230 |
| 53 | 109 | 152 | 183 | 249 |
| 58 | 118 | 155 | 185 | 251 |
| 60 | 130 | 156 | 186 | 265 |
| 62 | 136 | 160 | 198 | 286 |
| 63 | 138 | 161 | 200 | 289 |
| 66 | 140 | 162 | 203 | 292 |
| 69 | 148 | 163 | 206 | 293 |
| 300 | 324 | 364 | 393 | 419 |
| 301 | 328 | 365 | 394 | 422 |
| 304 | 329 | 371 | 396 | 423 |
| 305 | 330 | 375 | 398 | 427 |
| 309 | 332 | 376 | 399 | 428 |
| 313 | 342 | 380 | 401 | 430 |
| 315 | 346 | 381 | 403 | 434 |
| 318 | 351 | 382 | 404 | 441 |
| 322 | 359 | 385 | 407 | 442 |
| 323 | 360 | 387 | 415 | 444 |

These 100 manuscripts vary in length from 2 or 3 handwritten pages to as long as 222 sheets of 8 1/2 in. × 13 in. paper (for Paper 375).

An additional 38 sets of papers have thus far been identified only by year (judging from the date of material on the other side). These mostly date from 1905 to 1915.

There are perhaps 50 additional sets of papers not yet identified by either published paper or year.

## 4. MISCELLANEOUS MANUSCRIPTS

Cambridge Lectures Notes: Static Electricity and Magnetism; Sound; Light; Astronomy; Sound; Elementary Current Electricity; Advanced Electricity; B.A. Experiments.

Royal Institution Lecture Notes: Light (1889); Electricity (1890); Forces of Cohesion (1891); Matter at Rest and In Motion (1892); Sound (1893); Light (1894); Fluids (1895); Light (1896); Heat (1898); Capillarity (1899); Centenary Lecture "Young" (1899); Polarised Light (1900); Sound (1901); Shadows (1904).

Notes Found in Books: Author's copy of "Theory of Sound"; Author's copy of J.W.S. "Scientific Papers"; Maxwell's "Heat"; Maxwell's "Electricity"; Boussinesq's "Appl. des Potentiels", Thomson and Tait; Grey and Matthew's "Bessel Functions"; Coddington's "Optics"; Lamb's "Hydrodynamics"; Basset's "Hydrodynamics"; Shaw's "Forecasting Weather."

B4

Comments on Stokes' Wilde Lecture (1904).

Comments on Callender's Theory of Radiation.

Calculations relating to the "Theory of Sound": 8 folders.

Miscellaneous Notes on Electricity, 1883-1887.

Short manuscripts or rough calculations, all unpublsihed: 71 folders.

Correspondence and working papers pertaining to Trinity House.

## 5. SCIENTIFIC CORRESPONDENCE WITH LORD RAYLEIGH

Lord Rayleigh carefully saved his correspondence (including about half the time the envelope as well). When the fourth Rayleigh was preparing the biography of his father (1924), he sorted the scientific correspondence from the family correspondence and group the letters into approximate alphabetical order and three time periods: early to 1885; 1885-1896; 1897-1919. The Archives do not contain the originals of these letters, which the family wished to retain, but they have permitted us to index, microfilm, and publish them. As of this time only the indexing has been completed. Listed below are the names of the writers (sometimes difficult to decipher), the dates of the earliest and latest letters (occasionally unknown), and the number of letters from that correspondent. The Argon correspondence is not included here.

| | | | |
|---|---|---|---|
| Abel, F.A. | 1876 | Ames, J.S. | 1897 |
| Abney, W.W. | | Angstrom, K. | 1905 |
| Adams, J.C. | 1887-1888 (2) | Appleyard, R. | 1897 (2) |
| Adams, W.E. | 1879 | Armstrong, H.G. | 1892, 1894 (2) |
| Aichi, K. | 1918 (2) | Asquith, H.H. | 1908 |
| Airy, H. | 1883 | Austin, W.R. | 1895 |
| Aitken, J. | 1885-1917 (18) | Ayrton, H. | 1908 |
| Amagat, E.H. | 1904 | Ayrton, W.E. | 1884-1896 (4) |
| Baker, H.B. | 1902 | Boltzmann, Ludwig | 1884-1894 (4) |
| Balfour, A.J. | 1908, 1913 (2) | Borel, E. | 1913 |
| Bapan, J. | 1901 | Bosanquet, R.H.M. | (4) |
| Barket, G.F. | 1896 | Bose, J.C. | 1899 |
| Barus, C. | 1892-1904 (3) | Bottomley, J.T. | 1887-1888 (2) |
| Basset, A.B. | 1896 | Boys, C.V. | 1893-1918 (5) |
| Basset, M. | 1889 | Brace, D.B. | 1890 |
| Battelli, A. | 1901 | Bramwell, F. | 1886-1889 (3) |
| Batten, B. | 1901 | Brashear, J.A. | 1895-1905 (3) |
| Bell, Alex G. | 1878 | Brown, H. | 1898 |
| Benel, W. | 1897 (2) | Brown, H.T. | 1898-1910 (2) |
| Benham, C. | 1896 | Brown, W. | 1877 |
| Besant, W.S. | 1877 | Brown, W.G. | 1879 |
| Bidwell, S. | 1886 | Browning, J. | 1874 |
| Birkeland, C. | 1906 | Bryan, E.H. | 1895-1900 (2) |
| Bjerknes, V. | 1909 | Burbury, S.H. | 1891-1900 (2) |
| Blaikley, D.J. | 1884 | Bure, H.T. | 1898 |
| Blakesley, T.H. | 1891 | Burnside, W. | 1904-1917 (3) |
| Blocher, M. | 1899 | Butler, A.M. | 1875-1887 (3) |

| | | | | | |
|---|---|---|---|---|---|
| Caligny | 1883 | | Clarke, H. | 1885 | |
| Callendar, H. L. | 1913-1914 | (2) | Comins, A. T. | 1895 | |
| Cayley, A. | 1874-1888 | (3) | Common, A. A. | 1894 | |
| Chree, C. | 1889 | (2) | Cooper, R. E. S. | 1907 | |
| Christiansen, C. | 1892 | | Cornish, V. | 1907 | |
| Christie, W. H. | 1901 | | Cornu, A. | 1878-1902 | (5) |
| Chrystal, G. | 1880-1893 | (5) | Crookes, W. | 1888-1900 | (6) |
| Church, A. H. | 1892 | | Cunningham, A. W. | 1885 | |
| Clark, Latimer | 1889 | | Czapski, S. | 1888-1893 | (3) |
| Clarke, F. W. | 1894-1895 | (2) | | | |
| Darrth, W. F. | 1902 | | Dewar, J. | 1888-1901 | (6) |
| Darwin, G. H. | 1884-1912 | (7) | Dickson, J. D. H. | 1898 | |
| Devaux, H. | 1918 | | Dittmar, W. | 1890 | |
| Devonshire | 1881 | | Dorn, E. | 1893 | |
| Ebert, H. | 1889 | | Ettingshausen, A. | 1884 | |
| Eder, J. M. | 1895 | | Evans, W. | 1885 | (3) |
| Ehrenfest, P. | 1908 | | Everett, J. D. | 1871-1902 | (12) |
| Ellis, A. J. | 1876-1884 | (10) | Ewing, I. A. | 1886-1890 | (2) |
| Enock, J. K. | 1908-1912 | (2) | | | |
| Fabray, Ch. | 1915 | | Forsyth, A. R. | 1894 | |
| Ferrell, W. | 1889 | | Foster, G. Cary | 1878-1884 | (4) |
| Fessenden, R. A. | 1900-1901 | (2) | Foster, M. | 1906 | |
| Fitzgerald, G. F. | 1887-1893 | (6) | Frankland, E. | 1894-1895 | (4) |
| Fitzgerald, M. F. | 1899-1909 | (2) | French, J. W. | 1917 | |
| Flemming, J. A. | 1883-1901 | (5) | Frohlich, O. | 1891 | |
| Forbes, G. | 1882-1885 | (2) | Froude, R. E. | 1889-1913 | (10) |
| Forbes, M. | 1890 | | Froude, W. | 1876-1877 | (4) |
| Galloway, W. | 1901 | | Gore, Geo. | 1884 | |
| Galton, F. | 1876-1908 | (11) | Goschen | 1892 | |
| Gautier, A. | 1901 | | Graetz, L. | 1877 | (2) |
| Gibbs, J. W. | 1892-1900 | (4) | Gray, A. | 1886 | |
| Gladstone, W. E. | 1872 | | Green, F. W. | 1900 | |
| Glaisher, J. W. | 1872-1882 | (7) | Greenhide, G. | 1918 | |
| Glazebrook, R. T. | | (4) | Greenhill, A. G. | 1879-1885 | (3) |
| Gordon, G. | 1882 | | Griffiths, A. B. | 1906 | |
| Gordon, J. E. H. | 1881-1884 | (2) | Guye, P. A. | 1889-1912 | (8) |
| Hale, G. E. | 1901-1918 | (2) | Hinricks, G. | 1897 | |
| Harcourt, A. V. | 1886 | (2) | Hissong, W. | | |
| Hasselberg, B. | 1884 | (2) | Hodkinson, A. | 1888-1890 | (3) |
| Hazen, H. A. | 1892 | (2) | Hoffert, A. | 1889 | |
| Heaviside, O. | 1891-1913 | (5) | Holmes, O. W. | 1885 | |
| Helmholtz, H. von | 1885 | | Hopkinson, J. S. | 1884-1885 | (3) |
| Herman, R. A. | 1908 | | Horner, C. | 1876-1880 | (6) |
| Herrons, E. F. | 1892 | | Hough, S. S. | 1897 | |
| Herschel, A. S. | 1870-1878 | (5) | Huggins, W. | 1879-1891 | (9) |
| Hertz, H. | 1891 | | Humphrey, M. I. | 1904 | |
| Heycock, C. | 1898 | | Hunt, A. R. | 1875-1917 | (22) |
| Higgs, G. | 1890 | | Huxley, T. H. | 1896 | |
| Hill, E. A. | 1895 | (2) | | | |

| | | | | | |
|---|---|---|---|---|---|
| Ives, H.E. | 1905 | | Ives, J.E. | 1913 | |
| Jeans, J.H. | 1917 | (2) | Jonle, James | 1882 | |
| Johnson, G.L. | 1889 | | Judd, J.W. | 1888-1889 | (4) |
| Jones, J.V. | 1890 | | Juling, V.A. | 1889 | |
| Kayser, H. | 1899-1901 | (4) | Klemencick, I. | 1885 | |
| Kennelly, A.E. | 1912 | | Kohlrausch, F. | 1886-1908 | (5) |
| Kitasato, S. | 1908 | | Korteweg, D. | 1891-1912 | (4) |
| Labee, J.B. | 1875 | | Lockyer, N. | 1875 | (6) |
| Lamb, H. | 1887-1915 | (16) | Lodge, A. | 1876 | |
| Langley, S.P. | 1887-1891 | (2) | Lodge, O.J. | 1882-1908 | (31) |
| Lankester, E.R. | | (2) | Lommel, E. | 1887 | |
| Larmor, J. | 1896-1917 | (17) | Long, F.M. | 1913 | |
| Leduc, A. | 1898 | | Lorentz, H.A. | 1892-1905 | (5) |
| Lepinary, M. de | 1890 | | Loup, W.H. | 1897 | |
| Leudenfeld | 1902 | | Love, A.E.H. | 1888-1915 | (12) |
| Lewis, W.J. | 1904 | | Lowell, G.A. | 1884 | |
| Lippmann, G. | 1895-1896 | (2) | Lubend, J. | 1883-1888 | (3) |
| Lister, Jos. | 1894 | | Lummer, O. | 1900-1906 | (3) |
| Liuentaal, A. | 1901 | | Lyell, C. | 1868 | |
| Liveing, G.D. | 1879-1900 | (5) | Lyman, T. | 1904 | |
| Lloyd, R.J. | 1895-1896 | (5) | Lytton | 1888 | (2) |
| Mackenzie, H.W. | 1881 | | Meldola, R. | 1881-1890 | (3) |
| MacThanhon, P. | 1897 | | Mendeleef, D. | 1895-1905 | (2) |
| Mallock, A. | 1878-1914 | (16) | Merrisfield, C. | 1876 | |
| Mannheim | 1878 | | Meyer, L. | 1892 | |
| Marconi, G. | 1903-1909 | (2) | Michell, J.H. | 1890 | |
| Marangoni, C. | 1892 | | Michelson, A. | 1883-1912 | (18) |
| Marvin, C.F. | 1897 | | Minchin, E.A. | 1884-1893 | (2) |
| Mascart, E. | 1885-1899 | (14) | Monro, C.J. | 1871-1875 | (5) |
| Matthews, H. | 1890 | | Morgan, C.L. | 1890 | |
| Maxim, H. | 1900 | | Morley, W.M. | 1889 | |
| Maxwell, J.C. | 1871-1873 | (8) | Morton, W.B. | 1900-1906 | (3) |
| Mayer, A.M. | 1873-1894 | (10) | Muirhead, A. | 1886-1887 | (2) |
| McConnel, J.C. | 1887-1889 | (4) | Murtey, L. | 1896 | |
| McLeod, H. | 1877-1885 | (4) | Musprats, E. | 1888 | (5) |
| McMahon, J. | 1894 | (2) | Myers, J.W. | | |
| Natavson, L. | 1891 | | Newall, H.F. | 1885 | |
| Neesen, F. | 1882 | | Newcomb, S. | 1884-1903 | (6) |
| Nernst, W. | 1911 | | Norbert, F.A. | 1872 | (3) |
| Neumann, J. | 1882 | | Noyes, W.A. | 1890 | |
| Neville, L. | 1891 | | | | |
| Obach, E. | 1885 | (3) | Oosting, A.J. | 1888 | |
| Ohrbeck, A. | 1837 | | Orr, W.M. | 1900-1918 | (7) |
| Oliver, J.A.W. | 1882 | (2) | Ostwald, W. | 1902 | |
| Onnes, H. Kamerlingh | - 1900 | (2) | Owen, R. | 1886 | |

| | | |
|---|---|---|
| Parsons, C.S. | 1887-1908 | (2) |
| Parsons, L. (Rosse) | 1887 | |
| Pearson, K. | 1905 | |
| Peirce, C.S. | 1884 | |
| Perry, J. | 1889 | |
| Peterbrough, W.C. | 1875 | |
| Phillips, C. | 1909 | |
| Piccard, J. | 1891 | |
| Planck, M. | 1900 | |
| Playfair, L. | 1885 | |
| Pockels, A. | 1891 | (3) |
| Pockels, F. | 1891 | (2) |
| Pole, W. | 1890 | |
| Poynting, J.H. | 1883-1905 | (5) |
| Preese, W.W. | 1882-1889 | (4) |
| Preston, S T. | 1878-1883 | (6) |
| Preston, T. | 1894-1897 | (4) |
| Price, B. | 1888 | |
| Quincke, G. | 1874-1912 | (7) |
| Rae, J. | 1891 | |
| Ramsay, W. | 1886-1891 | (3) |
| Randall, W.W. | 1917 | |
| Reinold, A.W. | 1883-1885 | (2) |
| Reynolds, F.W. | 1882 | |
| Reynolds, Osborne | 1875-1886 | (5) |
| Richards, T.W. | 1892-1916 | (7) |
| Righi, A. | 1895 | |
| Rubens, H. | 1898-1910 | (3) |
| Rücker, A.W. | 1889 | |
| Russell, W.H.L. | 1872 | |
| Roberts, W.S. (Austen) | 1880-1888 | |
| Rohr, M. von | 1908 | |
| Roitz, A. | 1883 | |
| Rood, O.N. | 1898 | |
| Roscoe, H.E. | 1881-1883 | (3) |
| Rosse | 1885 | |
| Routh, E.I. | 1903 | (2) |
| Rowland, H. | 1882-1897 | (6) |
| Russell, W.J. | 1902 | |
| Rust, A. | 1890 | |
| Rutherford, E. | 1904 | |
| Salisbury | 1894 | (2) |
| Sampsin, R.A. | 1912-1913 | (3) |
| Schellen, A. | 1875-1878 | (2) |
| Schonrock, O. | 1906 | |
| Schuster, A. | 1880-1909 | (14) |
| Scott, A. | 1886-1887 | (2) |
| Scott, M. | 1885 | |
| Scott, R.H. | 1887 | |
| Seeman, R. | 1885 | |
| Shaw, H.H. | 1885 | |
| Shaw, W.N. | 1885 | |
| Sidgwick, E.M. | 1881 | |
| Sidgwick, H. | | |
| Siemens, C.W. | 1875-1882 | (3) |
| Sieming, W. | 1879-1884 | (4) |
| Simpson, A.B. | 1889-1890 | (8) |
| Simpson, J.C. | 1890 | |
| Sleeman, P. | 1884-1887 | |
| Smith, F.J. | 1891 | |
| Smith, W.H. | 1889 | |
| Smith, W.R. | 1882-1889 | (7) |
| Smyth, C.P. | 1877-1887 | (3) |
| Smyth, I. | 1887 | |
| Sommerfeld, A. | 1910-1912 | (2) |
| Sorby, H.C. | 1875-1878 | (3) |
| Southall, J.P.C. | 1911 | |
| Spotteswoode | 1882 | |
| Stanford, E.W. | 1888 | (3) |
| Stanton, T.I.F. | 1912 | |
| Stevens, W.L. | 1868-1889 | (4) |
| Stokes, G.G. | 1886-1887 | (3) |
| Stoney, E.A. | 1911 | |
| Stoney, G.J. | 1871-1905 | (7) |
| Strehike, F. | 1882 | |
| Stromeyer, C. | 1887-1896 | (3) |
| Strutt, R.J. | 1918 | |
| Suess, E. | 1895 | |
| Sutherland, W. | 1905 | |
| Swann, J.W. | 1896 | |
| Sylvester, J.J. | 1888 | (2) |
| Tait, P.G. | 1891-1902 | (16) |
| Tandusku, F.W. | 1909 | |
| Taylor, S. | 1893 | |
| Threlfall, R. | 1887-1909 | (5) |
| Thomas, D. | 1844 | |
| Thompson, S.P. | 1887-1888 | (3) |
| Thompson, W.H. | 1881-1885 | (3) |
| Thomson, J.J. | 1898-1905 | (6) |
| Thomson, James | | |
| Thomson, W. | 1884 | |
| Thorp, T. | 1902 | |
| Thorpe, G.S. | 1888-1917 | (2) |
| Thrustone, W.A. | 1886 | |
| Todhunter, J. | 1874-1878 | (5) |
| Tomlinson, C. | 1884 | |
| Tower, B. | 1875-1879 | (3) |
| Trotter, A.P. | 1889-1910 | |
| Trowbridge, J. | 1884 | |
| Tscherning | 1897-1902 | (4) |
| Tyndall, J. | 1874-1879 | (9) |

| | | | |
|---|---|---|---|
| Varley, S.A. | 1890 | Voight, W. | 1888-1910 (4) |
| Veley, V.H. | 1892 | Vyozan, G.R. | 1900 |
| Vines, S.H. | | | |
| Wadsworth, F.L.O. | 1896-1898 (5) | Webster, A.G. | 1900-1910 (5) |
| Walton | 1874-1877 (2) | Wells, H.G. | 1903 |
| Ward, J.T. | 1876-1886 (2) | Whitaker | 1896 |
| Wastuy, E. | 1908 | Whitmel, C.T. | 1889-1919 (5) |
| Watson, H.W. | 1833-1992 | Wiedemann, G. | 1882-1889 (7) |
| Weber, H. | 1900-1911 | Wien, Max | 1895 |
| Wild, F. | 1884 | Worthington, A.M. | 1889 |
| Williams, A.C. | 1918 | Wright, C.R. | 1883 |
| Williamson, A. | 1888 | Wright, L. | 1897-1900 (2) |
| Wilson, H.A. | 1908 | Young, S. | 1902 |
| Wilson, J.M. | 1884 | Zeeman, P. | 1897-1911 (4) |
| Wood, R.W. | 1898-1917 (35) | | |
| Wood, R.W. | (Rayleigh to) 1898-1919 (9) | | |

About 50 - signature illegible.

The total number of letters is about 1100.

## 4. THE ARGON CORRESPONDENCE

This consists of the Rayleigh-Ramsay letters, Ramsay-Dewar letters, about 100 sheets of manuscript calculations and data readings, and about 200 miscellaneous letters, mostly congratulations after the announcement of the discovery of argon.

### RAYLEIGH-RAMSAY CORRESPONDENCE

T.P. = Terling Place B.H. = Burlington House
A.G. = 12 Arundel Gardens U.C. = University College

| | | | |
|---|---|---|---|
| 22 January 1887 | R. to Ramsay | from 90 Onslow Gardens | (copy) |
| 7 July 1891 | R. to Ramsay | from B.H. | (copy) |
| 16 November 1891 | R. to Ramsay | from T.P. | (copy) |
| 20 November 1892 | Ramsay to R. | | |
| 15 March 1893 | R. to Ramsay | from B.H. | (copy) |
| 26 January 1894 | R. to Ramsay | from T.P. | (copy) |
| 24 May 1894 | Ramsay to R. | from A.G. | |
| 4 August 1894 | Ramsay to R. | from U.C. | |
| 7 August 1894 | R. to Ramsay | | |
| 7 August 1894 | Ramsay to R. | from A.G. | |
| 20 August 1894 | Ramsay to R. | from N.B. | |

| | | | |
|---|---|---|---|
| 24 August 1894 | Ramsay to R. | from N.B. | |
| 13 September 1894 | Ramsay to R. | from N.B. | |
| 16 September 1894 | R. to Ramsay | from T.P. | (copy) |
| 24 September 1894 | Ramsay to R. | from A.G. | |
| 27 September 1894 | Ramsay to R. | from A.G. | |
| 5 October 1894 | Ramsay to R. | from A.G. | |
| 13 October 1894 | Ramsay to R. | from U.C. | |
| 22 October 1894 | Ramsay to R. | from A.G. | |
| 28 October 1894 | Ramsay to R. | from A.G. | |
| 2 November 1894 | Ramsay to R. | from A.G. | |
| 7 November 1894 | Ramsay to R. | from A.G. | |
| 9 November 1894 | Ramsay to R. | from A.G. | |
| 15 November 1894 | Ramsay to R. | from U.C. | |
| 21 November 1894 | Ramsay to R. | from A.G. | |
| 29 November 1894 | Ramsay to R. | from A.G. | |
| 2 December 1894 | Ramsay to R. | from A.G. | |
| 3 December 1894 | Ramsay to R. | from A.G. | |
| 5 December 1894 | Ramsay to R. | | |
| 7 December 1894 | Ramsay to R. | from U.C. | |
| 8 December 1894 | Ramsay to R. | from A.G. | |
| 9 December 1894 | Ramsay to R. | from A.G. | |
| 17 December 1894 | Ramsay to R. | from U.C. | |
| 19 December 1894 | Ramsay to R. | from A.G. | |
| 24 December 1894 | Ramsay to R. | from A.G. | |
| 7 January 1895 | Ramsay to R. | from U.C. | |
| 11 January 1895 | Ramsay to R. | from A.G. | |
| 13 January 1895 | Ramsay to R. | from A.G. | |
| 14 January 1895 | Ramsay to R. | from U.C. | |
| Undated fragment | R. to Ramsay | | (copy) |
| 21 January 1895 | Ramsay to R. | from A.G. | |
| 28 January 1895 | Ramsay to R. | from U.C. | |
| 16 February 1895 | Ramsay to R. | from A.G. | |
| (Sunday A.M.) | Ramsay to R. | from A.G. | |
| 27 February 1895 | Ramsay to R. | from A.G. | |
| 18 March 1895 | Ramsay to R. | from A.G. | |
| 24 March 1895 | Ramsay to R. | from A.G. | |
| 25 March 1895 | Ramsay to R. | from A.G. | |
| 9 April 1895 | Ramsay to R. | from N.B. | |
| 20 May 1895 | Ramsay to R. | from A.G. | |
| 20 May 1895 | R. to Ramsay | from T.P. | (copy) |

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|---|---|---|---|
| 23 May 1895 | Ramsay to R. | from U.C. | |
| 25 May 1895 | Ramsay to R. | from A.G. | |
| 9 June 1895 | Ramsay to R. | from A.G. | |
| 18 June 1895 | Ramsay to R. | from U.C. | |
| 24 June 1895 | R. to Ramsay | from T.P. | (copy) |
| 25 July 1895 | Ramsay to R. | from U.C. | |
| 25 July 1895 | R. to Ramsay | from T.P. | (copy) |
| 25 July 1895 | Ramsay to R. | from A.G. | |
| 26 July 1895 | Ramsay to R. | from U.C. | |
| 13 August 1895 | R. to Mrs. Ramsay | from T.P. | (copy) |
| 26 August 1895 | R. to Ramsay | from T.P. | (copy) |
| 5 October 1895 | R. to Ramsay | from T.P. | (copy) |
| 18 December 1395 | R. to Ramsay | from 4 Carlton Gardens | (copy) |
| 4 January 1896 | R. to Ramsay | from T.P. | (copy) |
| 28 February 1896 | Ramsay to R. | from A.G. | |
| 3 March 1896 | R. to Ramsay | from 10 Downing Street | (copy) |
| 27 March 1896 | Ramsay to R. | from A.G. | |
| 13 July 1896 | Ramsay to R. | from A.G. | |
| 19 July 1896 | Ramsay to R. | from A.G. | |
| 2 October 1896 | Ramsay to R. | from A.G. | |
| 4 October 1896 | Ramsay to R. | from A.G. | |
| 10 November 1896 | R. to Ramsay | from T.P. | (copy) |
| 12 January 1897 | Ramsay to R. | from A.G. | |
| 14 January 1897 | R. to Ramsay | from Royal Institution | (copy) |
| 10 January 1897 | Ramsay to R. | from U.C. | |
| 13 May 1898 | Ramsay to R | from U.C. | |
| 23 May 1898 | R. to Ramsay | from T.P. | (copy) |
| 2 June 1898 | Ramsay to R. | from A.G. | |
| 5 June 1898 | R. to Ramsay | from Hatfield | (copy) |
| 7 June 1898 | Ramsay to R. | from A.G. | |
| 11 June 1898 | R. to Ramsay | from T.P. | |
| 13 June 1898 | Ramsay to R. | from A.G. | |
| 18 July 1898 | Ramsay to R. | from A.G. | |
| 21 July 1898 | R. to Ramsay | from T.P. | (copy) |
| 30 September 1898 | Ramsay to R. | from A.G. | |
| 28 November 1898 | R. to Ramsay | from T.P. | (copy) |
| 28 November 1898 | Ramsay to R. | from A.G. | |
| 2 December 1899 | Ramsay to R. | from A.G. | |
| 11 May 1900 | Ramsay to R. | from A.G. | |
| 15 May 1900 | Ramsay to R. | from U.C. | |

| | | | |
|---|---|---|---|
| 18 May 1900 | Ramsay to R. | from U.C. | |
| 24 February 1902 | R. to Ramsay | from 10 Downing Street | (copy) |
| 24 February 1902 | Ramsay to R. | from U.C. | |
| 11 March 1902 | Ramsay to R. | from U.C. | |
| 16 April 1902 | Ramsay to R. | from A.G. | |
| 20 November 1904 | R. to Ramsay | from T.P. | (copy) |
| 22 November 1904 | R. to Ramsay | from T.P. | |
| 2 December 1904 | R. to Ramsay | from T.P. | |
| 16 July 1907 | Ramsay to R. | from 19 Chester Terrace | |
| August 1906 | Lady Ramsay to R. | from High Wycombe | |
| 19 January 1917 | R. to Sir Wm.Tilden | from T.P. | |
| 7 March 1917 | R. to Sir Wm.Tilden | from 4 Carlton Gardens | (copy) |
| ? | Lady Ramsay to R.J. Ld. R. | from High Wycombe | |
| 7 October 1937 | M.W.Travers to R.J. Ld. R. | from Clifton, Bristol | |

(December 1966)

Unclassified
Security Classification

**DOCUMENT CONTROL DATA - R&D**

*(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)*

| | |
|---|---|
| 1 ORIGINATING ACTIVITY (Corporate author)<br>Hq AFCRL, OAR (CRN)<br>United States Air Force<br>Bedford, Massachusetts 01730 | 2a. REPORT SECURITY CLASSIFICATION<br>Unclassified<br>2b. GROUP |

3 REPORT TITLE

The Rayleigh Archives Dedication

4 DESCRIPTIVE NOTES (Type of report and inclusive dates)

Transcript of Rayleigh Archives Dedication Ceremony

5 AUTHOR(S) (Last name, first name, initial)

HOWARD, J.N. (Ed.)

| | | |
|---|---|---|
| 6 REPORT DATE<br>April 1967 | 7a TOTAL NO OF PAGES<br>91 | 7b NO. OF REFS<br>None |
| 8a CONTRACT OR GRANT NO. | 9a ORIGINATOR'S REPORT NUMBER(S)<br>AFCRL-67-0266<br>Special Reports No. 63 | |
| b PROJECT AND TASK NO. 8603-03 | | |
| c. DOD ELEMENT 61445014 | 9b OTHER REPORT NO(S) (Any other numbers that may be assigned this report)<br>AFCRL-67-0266 | |
| d. DOD SUBELEMENT 681310 | | |

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| 11 SUPPLEMENTARY NOTES | 12 SPONSORING MILITARY ACTIVITY<br>Hq AFCRL, OAR (CRN)<br>United States Air Force<br>Bedford, Massachusetts 01730 |

13 FORWARD

On 30 March 1966 a ceremony was held at the Research Library of the Air Force Cambridge Research Laboratories, Bedford, Massachusetts, to dedicate the Rayleigh Archives: a collection of the notebooks, manuscripts, working papers, and correspondence of the Lords Rayleigh (John William Strutt, the third Baron Rayleigh, and Robert John Strutt, the fourth Baron Rayleigh). The ceremony was attended by a distinguished group from both the military and the scientific community whose research interests relate to the two Rayleighs. The Strutt family was represented by the Honorable Charles R. Strutt (a son of the fourth Baron) and his wife. This report is a transcript of the discussions of "Rayleigh Day," with only a minimum of editing to correct minor errors that occurred in impromptu remarks. Appendices listing the attendees at the dedication ceremony and summarizing the contents of the Rayleigh Archives are included.

DD FORM 1 JAN 64 1473

Unclassified
Security Classification

Unclassified

Security Classification

| 14. KEY WORDS | LINK A | | LINK B | | LINK C | |
|---|---|---|---|---|---|---|
| | ROLE | WT | ROLE | WT | ROLE | WT |
| Rayleigh Scattering<br>Air Glow and Auroral Physics<br>History of Physics<br>Lord Rayleigh | | | | | | |

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